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LETTERS OF EULER

ON

NATURAL PHILOSOPHY.

EDITED BY

DAVID BREWSTER, LL.D.

IN TWO VOLUMES.

VOL. II.

WITH ENGRAVINGS.

New-York:

J. & J. HARPER, 82 CLIFF-STREET.

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1835.

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LETTERS OF EULER

ON DIFFERENT SUBJECTS IN

NATURAL PHILOSOPHY.

ADDRESSED TO

A GERMAN PRINCESS.

WITH NOTES, AND A LIFE OF EULER,

BY

DAVID BREWSTER, LL.D.

F.R.S. LOND. AND ED.

CONTAINING A GLOSSARY OF SCIENTIFIC TERMS.

WITH ADDITIONAL NOTES,

BY JOHN GRISCOM, LL.D.

IN TWO VOLUMES.

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LETTERS
ON
DIFFERENT SUBJECTS
IN
NATURAL PHILOSOPHY.

LETTER I.

Continuation of the Subject, and of Mistakes in the Knowledge of Truth.

The three classes of truths which I have now unfolded are the only sources of all our knowledge; all being derived from our own experience, from reasoning, or from the report of others.

It is not easy to determine which of these three sources contributes most to the increase of knowledge. Adam and Eve must have derived theirs chiefly from the two first; God, however, revealed many things to them, the knowledge of which is to be referred to the third source, as neither their own experience nor their powers of reasoning could have conducted them so far.

Without recurring to a period so remote, we are sufficiently convinced, that if we were determined to believe nothing of what we hear from others, or read in their writings, we should be in a state of almost total ignorance. It is very far, however, from being our duty to believe every thing that is said, or that we read. We ought constantly to employ our discerning faculties, not only with respect to truths of the third class, but likewise of the two others.

We are so liable to suffer ourselves to be dazzled by the senses, and to mistake in our reasonings, that the very sources laid open by the Creator for the discovery of truth very frequently plunge us into error. Notions of the third class, therefore, ought not in reason to fall under suspicion, any more than such as belong to the other two. We ought, therefore, to be equally on our guard against deception, whatever be the class to which the notion belongs; for we find as many instances of error in the first and second classes as in the third. The same thing holds with regard to the certainty of the particular articles of knowledge which these three sources supply; and it cannot be affirmed that the truths of any one order have a surer foundation than those of another. Each class is liable to errors, by which we may be misled; but there are likewise precautions which, carefully observed, furnish us with nearly the same degree of conviction. I do not know whether you are more thoroughly convinced of this truth, that two triangles which have the same base and the same height are equal to one another, than of this, that the Russians have been at Berlin; though the former is founded on a chain of accurate reasoning, whereas the latter depends entirely on the veracity of your informer.

Respecting the truths, therefore, of each of these classes, we must rest satisfied with such proofs as correspond to their nature; and it would be ridiculous to insist upon a geometrical demonstration of the truths of experience, or of history. This is usually the fault of those who make a bad use of their penetration in intellectual truths, to require mathematical demonstration in proof of all the truths of religion, a great part of which belongs to the third class.

There are persons determined to believe and admit nothing but what they see and touch; whatever you would prove to them by reasoning, be it ever so solid, they are disposed to suspect, unless you place it before their eyes. Chymists, anatomists, and natural philosophers, who employ themselves wholly in making experiments, are most chargeable with this fault. Every thing that the one cannot melt in his crucible, or the other dissect with his scalpel, they reject as unfounded. To no purpose would you speak to them of the qualities and nature of the soul; they admit nothing but what strikes the senses.

Thus, the particular kind of study to which every one is addicted has such a powerful influence on his manner of thinking, that the natural philosopher and chymist will have nothing but experiments, and the geometrician and logician nothing but arguments; which constitute, however, proofs entirely different, the one attached to the first class, the other to the second, which ought always to be carefully distinguished, according to the nature of the objects.

But can it be possible that persons should exist who, wholly absorbed in pursuits pertaining to the third class, call only for proofs derived from that source? I have known some of this description, who, totally devoted to the study of history and antiquity, would admit nothing as true but what you could prove by history, or the authority of some ancient author. They perfectly agree with you respecting the truth of the propositions of Euclid, but merely on the authority of that author, without paying any attention to the demonstrations by which he supports them; they even imagine that the contrary of these propositions might be true, if the ancient geometricians had thought proper to maintain it.

This is a source of error which retards many in the pursuit of truth; but we find it rather among the learned, than among those who are beginning to

apply themselves to the study of the sciences. We ought to have no predilection in favour of any one of the three species of proofs which each class requires; and provided they are sufficient in their kind, we are bound to admit them.

I have seen or felt, is the proof of the first class. *I can demonstrate it*, is that of the second: we likewise say, *I know it is so*. Finally, *I receive it on the testimony of persons worthy of credit*, or *I believe it on solid grounds*, is the proof of the third class.

4th April, 1761.

LETTER II.

First Class of known Truths. Conviction that Things exist externally, corresponding to the Ideas represented by the Senses. Objection of the Pyrrhonists. Reply.

We include in the first class of known truths those which we acquire immediately by means of the senses. I have already remarked, that they not only supply the soul with certain representations relative to the changes produced in a part of the brain; but that they excite there a conviction of the real existence of things external, corresponding to the ideas which the senses present to us.

The soul is frequently compared to a man shut up in a dark room, in which the images of external objects are represented on the wall by means of a glass. This comparison is tolerably just, as far as it respects the man looking at the images on the wall; for this act is sufficiently similar to that of the soul, contemplating the impressions made in the brain; but the comparison appears to me extremely defective, as far as it respects the conviction that the objects which occasion these images really exist.

The man in the dark room will immediately suspect the existence of these objects; and if he has no doubt about the matter, it is because he has been out of doors, and has seen them; besides this, knowing the nature of his glass, he is assured that nothing can be represented on the wall but the images of the objects which are without the chamber before the glass. But this is not the case with the soul; it has never quitted its place of residence to contemplate the objects themselves; and it knows still less the construction of the sensitive organs, and the nerves which terminate in the brain. It is nevertheless much more powerfully convinced of the real existence of objects than our man in the dark room possibly can be. I am apprehensive of no objection on this subject, the thing being too clear of itself to admit any, though we do not know the true foundation of it. No one ever entertained any doubt about it, except certain visionaries who have bewildered themselves in their own reveries. Though they say that they doubt the existence of external objects, they entertain no such doubt in fact; for why would they have affirmed it, unless they had believed the existence of other men, to whom they wished to communicate their extravagant opinions.

This conviction respecting the existence of the things whose images the senses represent, appears not only in men of every age and condition, but likewise in all animals. The dog which barks at me has no doubt of my existence, though his soul perceives but a slight image of my person. Hence I conclude, that this conviction is essentially connected with our sensations, and that the truths which the senses convey to us are as well founded as the most undoubted truths of geometry.

Without this conviction no human society could subsist, for we should be continually falling into the greatest absurdities, and the grossest contradictions. Were the peasantry to dream of doubting about the existence of their bailiff, or soldiers about that of their officers, into what confusion should we be plunged! Such absurdities are entertained only by philosophers; any other giving himself up to them would be considered as having lost his reason. Let us then acknowledge this conviction as one of the principal laws of nature, and that it is complete, though we are absolutely ignorant of its true reasons, and very far from being able to explain them in an intelligible manner.

However important this reflection may be, it is by no means, however, exempted from difficulties; but were they ever so great, and though it might be impossible for us to solve them, they do not in the smallest degree affect the truth which I have just established, and which we ought to consider as the most solid foundation of human knowledge.

It must be allowed that our senses sometimes deceive us; and hence it is that those subtile philosophers who value themselves on doubting of every thing deduce the consequence, that we ought never to depend on our senses. I have perhaps oftener than once met an unknown person in the street, whom I mistook for an acquaintance: as I was deceived in that instance, nothing prevents my being always deceived; and I am, therefore, never assured that the person to whom I speak is in reality the one I imagine.

Were I to go to Magdeburg, and to present myself to your highness, I ought always to be apprehensive of grossly mistaking: nay, perhaps I should not be at Magdeburg, for there are instances of a man's sometimes taking one city for another. It is even possible I may never have had the happiness of seeing you, but was always under the power of delusion when I thought myself to be enjoying that felicity.

Such are the natural consequences resulting from the sentiments of certain philosophers; and you must be abundantly sensible that they not only lead to manifest absurdity, but have a tendency to dissolve all the bonds of society.

7th April, 1761.

LETTER III.

Another Objection of the Pyrrhonists against the Certainty of Truths perceived by the Senses. Reply; and Precautions for attaining Assurance of Sensible Truths.

Though the objection raised against the certainty of truths perceived by the senses, of which I have been speaking, may appear sufficiently powerful, attempts have been made to give it additional support from the well-known maxim, that we ought never to trust him who has once deceived us. A single example, therefore, of mistake in the senses, is sufficient to destroy all their credit. If this objection is well founded, it must be admitted that human society is, of course, completely subverted.

By way of reply, I remark, that the two other sources of knowledge are subject to difficulties of a similar nature, nay, perhaps still more formidable. How often are our reasonings erroneous! I venture to affirm, that we are much more frequently deceived by these than by our senses. But does it follow that our reasonings are always fallacious, and that we can have no dependence on any truth discovered to us by the understanding? It must be a matter of doubt, then, whether two and two make four, or whether the three angles of a triangle be equal to two right angles; it would even be ridiculous to pretend that this should pass for truth. Though, therefore, men may have frequently reasoned inconclusively, it would be almost absurd to

infer that there are not many intellectual truths of which we have the most complete conviction.

The same remark applies to the third source of human knowledge, which is unquestionably the most subject to error. How often have we been deceived by a groundless rumour, or false report, respecting certain events! And who would be so weak as to believe all that gazetteers and historians have written? At the same time, whoever should think of maintaining that every thing related or written by others is false would undoubtedly fall into greater absurdities than the person who believed every thing. Accordingly, notwithstanding so many groundless reports and false testimonies, we are perfectly assured of the truth of numberless facts, of which we have no evidence but testimony.

There are certain characters which enable us to distinguish truth; and each of the three sources has characters peculiar to itself. When my eyes have deceived me, in mistaking one man for another, I presently discover my error: it is evident, therefore, that precautions may be used for the prevention of error. If there were not, it would be impossible ever to perceive that we had been deceived. Those, then, who maintain that we so often deceive ourselves are obliged to admit that it is possible for us to perceive we have been deceived, or they must acknowledge that they themselves are deceived when they charge us with error.

It is remarkable, that truth is so well established that the most violent propensity to doubt of every thing must come to this, in spite of itself. Therefore, as logic prescribes rules for just reasoning, the observance of which will secure us from error, where intellectual truth is concerned; there are likewise certain rules, as well for the first source, that of our senses, as for the third, that of belief

The rules of the first are so natural to us, that all men, the most stupid not excepted, understand and practise them much better than the greatest scholars are able to describe them. Though it may be easy sometimes to confound a clown, yet when the hail destroys his crop, or the thunder breaks upon his cottage, the most ingenious philosopher will never persuade him that it was a mere illusion; and every man of sense must admit that the country-fellow is in the right, and that he is not always the dupe of the fallaciousness of his senses. The philosopher may be able, perhaps, to perplex him to such a degree that he shall be unable to reply; but he will inwardly treat all the fine reasonings which attempted to confound him with the utmost scorn. The argument, that the senses sometimes deceive us, will make but a very slight impression on his mind; and when he is told, with the greatest eloquence, that every thing the senses represent to us has no more reality than the visions of the night, it will only provoke laughter.

But if the clown should pretend to play the philosopher in his turn, and maintain that the bailiff is a mere phantom, and that all who consider him as something real, and submit to his authority, are fools; this sublime philosophy would be in a moment overturned, and the leader of the sect soon made to feel, to his cost, the force of the proofs which the bailiff could give him of the reality of his existence.

You must be perfectly satisfied, then, that there are certain characters which destroy every shadow of doubt respecting the reality and truth of what we know by the senses; and these same characters are so well known, and so strongly impressed on our minds, that we are never deceived when we employ the precautions necessary to that effect. But it is extremely difficult to make an exact enumeration of these characters, and to explain their nature. We commonly say, that the sensitive organs ought to be in a good natural state; that the air ought not to be obscured by a fog; finally, that we must employ a sufficient degree of attention, and endeavour, above all things, to examine the same object by two or more of our senses at once. But I am firmly persuaded that every one knows, and puts in practice, rules much more solid than any which could be prescribed to him.

11*th April*, 1761.

LETTER IV.

Of Demonstrative, Physical, and particularly of Moral Certainty.

There are, therefore, three species of knowledge which we must consider as equally certain, provided we employ the precautions necessary to secure us against error. And hence likewise result three species of certainty.

The first is called *physical certainty.* When I am convinced of the truth of any thing, because I myself have seen it, I have a physical certainty of it; and if I am asked the reason, I answer, that my own senses give me full assurance of it, and that I am, or have been, an eyewitness of it. It is thus I know that Austrians have been at Berlin, and that some of them committed great irregularities there. I know, in the same manner, that fire consumes all combustible substances; for I myself have seen it, and I have a physical certainty of its truth.

The certainty which we acquire by a process of reasoning is called *logical* or *demonstrative certainty,* because we are convinced of its truth by demonstration. The truths of geometry may here be produced as examples, and it is logical certainty which gives us the assurance of them.

Finally, the certainty which we have of the truth of what we know only by the report of others is called *moral certainty*, because it is founded on the *credibility* of the persons who make the report. Thus you have only a moral certainty that the Russians have been at Berlin; and the same thing applies to all historical facts. We know with a moral certainty that there was formerly at Rome a Julius Cæsar, an Augustus, a Nero, &c., and the testimonies respecting these are so authentic, that we are as fully convinced of them as of the truths which we discover by our senses, or by a chain of fair reasoning.

We must take care, however, not to confound these three species of certainty—physical, logical, and moral—each of which is of a nature totally different from the others. I propose to treat of each separately; and shall begin with a more particular explanation of moral certainty, which is the third species.

It is to be attentively remarked, that this third source divides into two branches, according as others simply relate what they themselves have seen, or made full proof of by their senses, or as they communicate to us, together with these, their reflections and reasonings upon them. We might add still a third branch, when they relate what they have heard from others.

As to this third branch, it is generally allowed to be very liable to error, and that a witness is to be believed only respecting what he himself has seen or experienced. Accordingly, in courts of justice, when witnesses are examined, great care is taken to distinguish, in their declarations, what they themselves have seen and experienced, from what they frequently add of their reflections and reasonings upon it. Stress is laid only on what they themselves have seen or experienced; but their reflections, and the conclusions which they draw, however well founded they may otherwise be, are entirely set aside. The same maxim is observed with respect to historians;

and we wish them to relate only what they themselves have witnessed, without pursuing the reflections which they so frequently annex, though these may be a great ornament to history. Thus we have a greater dependence on the truth of what others have experienced by their own senses, than on what they have discovered by pursuing their meditations. Every one wishes to be master of his own judgment; and unless he himself feels the foundation and the demonstration, he is not persuaded.

Euclid would in vain have announced to us the most important truths of geometry; we should never have believed him on his word, but have insisted on prosecuting the demonstration step by step ourselves. If I were to tell you that I had seen such or such a thing, supposing my report faithful, you would without hesitation give credit to it; nay, I should be very much mortified if you were to suspect me of falsehood. But when I inform you that in a right-angled triangle, the squares described on the two smaller sides are together equal to the square of the greater side, I do not wish to be believed on my word, though I am as much convinced of it as it is possible to be of any thing; and though I could allege, to the same purpose, the authority of the greatest geniuses who have had the same conviction, I should rather wish you to discredit my assertion, and to withhold your assent, till you yourself comprehended the solidity of the reasonings on which the demonstration is founded.

It does not follow, however, that physical certainty, or that which the senses supply, is greater than logical certainty, founded on reasoning; but whenever a truth of this species presents itself, it is proper that the mind should give close application to it, and become master of the demonstration. This is the best method of cultivating the sciences, and of carrying them to the highest degree of perfection.

The truths of the senses, and of history, greatly multiply the particulars of human knowledge; but the faculties of the mind are put in action only by reflection or reasoning.

We never stop at the simple evidence of the senses, or the facts related by others; but always follow them up and blend them with reflections of our own: we insensibly supply what seems deficient, by the addition of causes and motives, and the deduction of consequences. It is extremely difficult, for this reason, in courts of justice, to procure simple unblended testimony, such as contains what the witnesses actually saw and felt, and no more; for witnesses ever will be mingling their own reflections, without perceiving that they are doing so.

14*th April*, 1761.

LETTER V.

Remarks that the Senses contribute to the Increase of Knowledge; and Precautions for acquiring the Certainty of Historical Truths.

The knowledge supplied by our senses is undoubtedly the earliest which we acquire; and upon this the soul founds the thoughts and reflections which discover to it a great variety of intellectual truths. In order the better to comprehend how the senses contribute to the advancement of knowledge, I begin with remarking, that the senses act only on individual things, which actually exist under circumstances determined or limited on all sides.

Let us suppose a man suddenly placed in the world, possessed of all his faculties, but entirely destitute of experience; let a stone be put in his hand, let him then open that hand, and observe that the stone falls. This is an experiment limited on all sides, which gives him no information, except that this stone, being in the left hand, for example, and dropped, falls to the ground; he is by no means absolutely certain that the same effect would ensue were he to take another stone, or the same stone, with his right hand. It is still uncertain whether this stone, under the same circumstances, would again fall, or whether it would have fallen had it been taken up an hour sooner. This experiment alone gives him no light respecting these particulars.

The man in question takes another stone, and observes that it falls likewise, whether dropped from the right hand or from the left: he repeats the experiment with a third and a fourth stone, and uniformly observes the same effect. He hence concludes that stones have the property of falling when dropped, or when that which supports them is withdrawn.

Here then is an article of knowledge which the man has derived from the experiments which he has made. He is very far from having made trial of every stone, or, supposing him to have done so, what certainty has he that the same thing would happen at all times? He knows nothing as to this, except what concerns the particular moments when he made the experiments; and what assurance has he that the same effect would take place in the hands of another man? Might he not think that this quality of making stones fall was attached to his hands exclusively? A thousand other doubts might still be formed on the subject.

I have never, for example, made trial of the stones which compose the cathedral church of Magdeburg, and yet I have not the least doubt that all of them, without exception, are heavy, and that each of them would fall as soon as detached from the building. I even imagine that experience has supplied me with this knowledge, though I have never tried any one of those stones.

This example is sufficient to show how experiments made on individual objects only have led mankind to the knowledge of universal propositions; but it must be admitted that the understanding and the other faculties of the soul interfere in a manner which it would be extremely difficult clearly to unfold; and if we were determined to be over-scrupulous about every circumstance, no progress in science could be made, for we should be stopped short at every step.

It must be allowed, that the vulgar discover in this respect much more good sense than those scrupulous philosophers who are obstinately determined to doubt of every thing. It is necessary, at the same time, to be on our guard against falling into the opposite extreme, by neglecting to employ the necessary precautions.

The three sources from which our knowledge is derived require all of them certain precautions, which must be carefully observed, in order to acquire assurance of the truth; but it is possible, in each, to carry matters too far, and it is always proper to steer a middle course.

The third source clearly proves this. It would undoubtedly be extreme folly to believe every thing that is told us; but excessive distrust would be no less blameworthy. He who is determined to doubt of every thing will never want a pretence; when a man says or writes that he has seen such or such an action, we may say at once that it is not true, and that the man takes amusement in relating things which may excite surprise; and if his veracity is beyond suspicion, it might be said that he did not see clearly, that his eyes were dazzled; and examples are to be found in abundance of persons deceiving themselves, falsely imagining they saw what they did not. The rules prescribed in this respect lose all their weight when you have to do with a wrangler.

Usually, in order to be ascertained of the truth of a recital or history, it is required that the author should have been himself a witness of what he relates, and that he should have no interest in relating it differently from the truth. If afterward two or more persons relate the same thing, with the same circumstances, it is justly considered as a strong confirmation. Sometimes, however, a coincidence carried to extreme minuteness becomes suspicious. For two persons observing the same incident see it in different points of view; and the one will always discern certain little circumstances which the other must have overlooked. A slight difference in two several accounts of the same event rather establishes than invalidates the truth of it.

But it is always extremely difficult to reason on the first principles of our knowledge, and to attempt an explanation of the mechanism and of the moving powers which the soul employs. It would be glorious to succeed in such an attempt, as it would elucidate a great variety of important points respecting the nature of the soul and its operations. But we seem destined rather to make use of our faculties, than to trace their nature through all its depths.

18th April, 1761.

LETTER VI.

Whether the Essence of Bodies be known by us.

After so many reflections on the nature and faculties of the soul, you will not perhaps be displeased to return to the consideration of body, the principal properties of which I have already endeavoured to explain.

I have remarked, that the nature of body necessarily contains three things, *extension*, *impenetrability*, and *inertia*; so that a being in which these three properties do not meet at once cannot be admitted into the class of bodies; and reciprocally, when they are united in any one being, no one will hesitate to acknowledge it for a body.

In these three things, then, we are warranted to constitute the essence of body, though there are many philosophers who pretend that the essence of bodies is wholly unknown to us. This is not only the opinion of the Pyrrhonists, who doubt of every thing; but there are other sects likewise who maintain that the essence of all things is absolutely unknown: and, no doubt, in certain respects they have truth on their side: this is but too certain as to all the individual beings which exist.

You will easily comprehend, that it would be the height of absurdity were I to pretend so much as to know the essence of the pen which I employ in writing this Letter. If I knew the essence of this pen (I speak not of pens in general, but of that one only now between my fingers, which is an *individual being*, as it is called in metaphysics, and which is distinguished from all the other pens in the world), if I knew, then, the essence of this individual pen, I should be in a condition to distinguish it from every other, and it would be impossible to change it without my perceiving the change; I must know its nature thoroughly, the number and the arrangement of all the parts whereof it is composed. But how far am I from having such a knowledge! Were I to rise but for a moment, one of my children might easily change it, leaving another in its room, without my perceiving the difference; and were I even to put a mark upon it, how easily might that mark be counterfeited on another pen. And supposing this impossible for my children, it must always be admitted as possible for God to make another pen so similar to this that I should be unable to discern any difference. It would be, however, another pen, really distinguishable from mine, and God would undoubtedly know the difference of them; in other words, God perfectly knows the essence of both the one and the other of these two pens: but as to me, who discern no difference, it is certain that the essence is altogether beyond my knowledge.

The same observation is applicable to all other individual things; and it may be confidently maintained, that God alone can know the essence or nature of each. It were impossible to fix on any one thing really existing of which we could have a knowledge so perfect as to put us beyond the reach of mistake: this is, if I may use the expression, the impress of the Creator on all created things, the nature of which will ever remain a mystery to us.

It is undoubtedly certain, then, that we do not know the essence of individual things, or all the characters whereby each is distinguished from every other; but the case is different with respect to *genera* and *species:* these are general notions which include at once an infinite number of individual things. They are not beings actually existing, but notions which we ourselves form in our minds when we arrange a great many individual things in the same class, which we denominate a species or genus, according as the number of individual things which it comprehends is greater or less.

And to return to the example of the pen, as there are an infinite number of things to each of which I give the same name, though they all differ one from another, the notion of *pen* is a general idea, of which we ourselves are the creators, and which exists only in our own minds. This notion contains but the common characters which constitute the essence of the general notion of a pen; and this essence must be well known to us, as we are in a condition to distinguish all the things which we call *pens* from those which we do not comprehend under that appellation.

As soon as we remark in any thing certain char acters, or certain qualities, we say it is a pen: and we are in a condition to distinguish it from all other things which are not pens, though we are very far from being able to distinguish it from other pens.

The more general a notion is, the fewer it contains of the characters which constitute its essence; and it is accordingly easier also to discover this essence. We comprehend more easily what is meant by a tree in general than by the term cherry-tree, pear-tree, or apple-tree; that is, when we descend to the species. When I say such an object which I see in the garden is a tree, I run little risk of being mistaken; but it is extremely possible I might be wrong if I affirmed it was a cherry-tree. It follows, then, that I know much better the essence of tree in general than of the species; I should not so easily confound a tree with a stone as a cherry-tree with a plum-tree.

Now a notion in general extends infinitely further; its essence accordingly comprehends only the characters which are common to all beings bearing the name of *bodies*. It is reduced, therefore, to a very few particulars, as we must exclude from it all the characters which distinguish one body from another.

It is ridiculous, then, to pretend with certain philosophers that the essence of bodies in general is unknown to us. If it were so, we should never be in a condition to affirm with assurance that such a thing is a body, or it is not; and as it is impossible we should be mistaken in this respect, it necessarily follows that we know sufficiently the nature or essence of body in general. Now this knowledge is reduced to three articles: extension, impenetrability, and inertia.

21*st April*, 1761.

LETTER VII.

The True Notion of Extension.

I HAVE already demonstrated that the general notion of body necessarily comprehends these three qualities, extension, impenetrability, and inertia, without which no being can be ranked in the class of bodies. Even the most scrupulous must allow the necessity of these three qualities in order to constitute a body; but the doubt with some is, Are these three characters sufficient? Perhaps, say they, there may be several other characters which are equally necessary to the essence of body.

But I ask, were God to create a being divested of these other unknown characters, and that it possessed only the three above mentioned, would they hesitate to give the name of body to such a being? No, assuredly; for if they had the least doubt on the subject, they could not say with certainty that the stones in the street are bodies, because they are not sure whether the pretended unknown characters are to be found in them or not.

Some imagine that gravity is an essential property of all bodies, as all those which we know are heavy; but were God to divest them of gravity, would they therefore cease to be bodies? Let them consider the heavenly bodies, which do not fall downward; as must be the case if they were heavy as the bodies which we touch, yet they give them the same name. And even on the supposition that all bodies were heavy, it would not follow that gravity is a property essential to them, for a body would still remain a body, though its gravity were to be destroyed by a miracle.

But this reasoning does not apply to the three essential properties above mentioned. Were God to annihilate the extension of a body, it would certainly be no longer a body; and a body divested of impenetrability would no longer be *body;* it would be a spectre, a phantom: the same holds as to inertia.

You know that extension is the proper object of geometry, which considers bodies only in so far as they are extended, abstractedly from impenetrability and inertia; the object of geometry, therefore, is a notion much more general than that of body, as it comprehends, not only bodies, but all things simply extended, without impenetrability, if any such there be. Hence it follows that all the properties deduced in geometry from the notion of extension must likewise take place in bodies, inasmuch as they are extended; for whatever is applicable to a more general notion, to that of a tree, for example, must likewise be applicable to the notion of an oak, an ash, an elm, &c.; and this principle is even the foundation of all the reasonings in virtue of which we always affirm and deny of the species, and of individuals, every thing that we affirm and deny of the genus.

There are however philosophers, particularly among our contemporaries, who boldly deny that the properties applicable to extension in general, that is, according as we consider them in geometry, take place in bodies really existing. They allege that geometrical extension is an abstract being, from the properties of which it is impossible to draw any conclusion with respect to real objects; thus, when I have demonstrated that the three angles of a triangle are together equal to two right angles, this is a property belonging only to an abstract triangle, and not at all to one really existing.

But these philosophers are not aware of the perplexing consequences which naturally result from the difference which they establish between objects formed by abstraction and real objects; and if it were not permitted to conclude from the first to the last, no conclusion, and no reasoning whatever, could subsist, as we always conclude from general notions to particular.

Now all general notions are as much abstract beings as geometrical extension; and a tree in general, or the general notion of trees, is formed only by abstraction, and no more exists out of our mind than geometrical extension does. The notion of man in general is of the same kind, and man in general nowhere exists: all men who exist are individual beings, and correspond to individual notions. The general idea which comprehends all is formed only by abstraction.

The fault which these philosophers are ever finding with geometricians, for employing themselves about abstractions merely, is therefore groundless, as all other sciences principally turn on general notions, which are no more real than the objects of geometry. The patient, in general, whom the physician has in view, and the idea of whom contains all patients really existing, is only an abstract idea; nay, the very merit of each science is so much the greater, as it extends to notions more general, that is to say, more abstract.

I shall endeavour by next post to point out the tendency of the censures pronounced by these philosophers upon geometricians; and the reasons why they are unwilling that we should ascribe to real extended beings, that is, to existing bodies, the properties applicable to extension in general, or to abstracted extension. They are afraid lest their metaphysical principles should suffer in the cause.

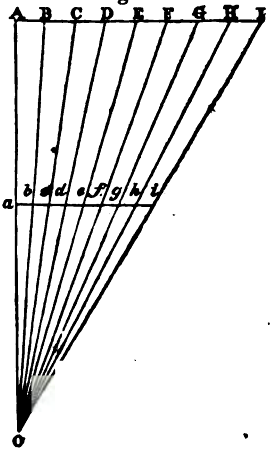
25th April, 1761.

LETTER VIII.

Divisibility of Extension in infinitum.

The controversy between modern philosophers and geometricians, to which I have alluded, turns on the divisibility of body. This property is undoubtedly founded on extension; and it is only in so far as bodies are extended that they are divisible, and capable of being reduced to parts.

Fig. 38.



You will recollect that in geometry it is always possible to divide a line, however small, into two equal parts. We are likewise by that science instructed in the method of dividing a small line, as *a i*, *Fig.* 38, into any number of equal parts at pleasure: and the construction of this division is there demonstrated beyond the possibility of doubting its accuracy.

You have only to draw a line A I parallel to *a i* of any length, and at any distance you please, and to divide it into as many equal parts AB, BC, CD, DE, &c. as the small line given is to have divisions, say eight. Draw afterward, through the extremities A *a*, and I *i*, the straight lines A *a* O, I *i* O, till they meet in the point O; and from O draw towards the points of division B, C, D, E, &c. the straight lines OB, OC, OD, OE,

&c., which shall likewise divide the small line $a\,i$ into eight equal parts.

This operation may be performed, however small the given line $a\,i$, and however great the number of parts into which you propose to divide it. It is true that in execution we are not permitted to go too far; the lines which we draw have always some breadth, whereby they are at length confounded, as may be seen in the figure near the point O; but the question is, not what may be possible for us to execute, but what is possible in itself. Now, in geometry lines have no breadth, and consequently can never be confounded. Hence it follows that such division is illimitable.

If it is once admitted that a line may be divided into a thousand parts, by dividing each part into two it will be divisible into two thousand parts, and for the same reason into four thousand, and into eight thousand, without ever arriving at parts indivisible. However small a line may be supposed, it is still divisible into halves, and each half again into two, and each of these again in like manner, and so on to infinity.

What I have said of a line is easily applicable to a surface, and, with greater strength of reasoning, to a solid endowed with three dimensions,—length, breadth, and thickness. Hence it is affirmed that all extension is divisible to infinity; and this property is denominated *divisibility in infinitum.*

Whoever is disposed to deny this property of extension is under the necessity of maintaining that it is possible to arrive at last at parts so minute as to be unsusceptible of any further division, because they cease to have any extension. Nevertheless, all these particles taken together must reproduce the whole, by the division of which you acquired them; and as the quantity of each would be a *nothing* or *cipher* 0, a combination of ciphers would produce quantity, which is manifestly absurd. For you know perfectly well that in arithmetic two or more ciphers joined never produce any thing.

This opinion, that in the division of extension or of any quantity whatever, we may come at last to particles so minute as to be no longer divisible, because they are so small, or because quantity no longer exists, is therefore a position absolutely untenable.

In order to render the absurdity of it more sensible, let us suppose a line of an inch long divided into a thousand parts, and that these parts are so small as to admit of no further division; each part, then, would no longer have any length, for if it had any it would be still divisible. Each particle, then, would of consequence be a nothing. But if these thousand particles together constituted the length of an inch, the thousandth part of an inch would of consequence be a nothing; which is equally absurd with maintaining that the half of any quantity whatever is nothing. And if it be absurd to affirm that the half of any quantity is nothing, it is equally so to affirm that the half of a half, or that the fourth part of the same quantity is nothing; and what must be granted as to the fourth must likewise be granted with respect to the thousandth and the millionth part. Finally, however far you may have already carried in imagination the division of an inch, it is always possible to carry it still further; and never will you be able to carry on your subdivision so far as that the last parts shall be absolutely indivisible. These parts will undoubtedly always become smaller, and their magnitude will approach nearer and nearer to 0, but can never reach it.

The geometrician, therefore, is warranted in affirming that every magnitude is divisible to infinity; and that you cannot proceed so far in your division as that all further division shall be impossible. But it is always necessary to distinguish between what is possible in itself and what we are in a condition

to perform. Our execution is indeed extremely limited. After having, for example, divided an inch into a thousand parts, these parts are so small as to escape our senses; and a further division would to us no doubt be impossible.

But you have only to look at this thousandth part of an inch through a good microscope, which magnifies, for example, a thousand times, and each particle will appear as large as an inch to the naked eye; and you will be convinced of the possibility of dividing each of these particles again into a thousand parts: the same reasoning may always be carried forward without limit and without end.

It is therefore an indubitable truth that all magnitude is divisible *in infinitum;* and that this takes place not only with respect to extension, which is the object of geometry, but likewise with respect to every other species of quantity, such as time and number.

28*th April*, 1761.

LETTER IX.

Whether this Divisibility in infinitum takes place in existing Bodies.

It is then a completely established truth, that extension is divisible to infinity, and that it is impossible to conceive parts so small as to be unsusceptible of further division. Philosophers accordingly do not impugn this truth itself, but deny that it takes place in existing bodies. They allege that extension, the divisibility of which to infinity has been demonstrated, is merely a chimerical object, formed by abstraction; and that simple extension, as considered in geometry, can have no real existence.

Here they are in the right; and extension is undoubtedly a general idea, formed in the same manner as that of man, or of tree in general, by abstraction; and as man or tree in general does not exist, no more does extension in general exist. You are perfectly sensible that individual beings alone exist, and that general notions are to be found only in the mind; but it cannot therefore be maintained that these general notions are chimerical; they contain, on the contrary, the foundation of all our knowledge.

Whatever applies to a general notion, and all the properties attached to it, of necessity takes place in all the individuals comprehended under that general notion. When it is affirmed that the general notion of man contains an understanding and a will, it is undoubtedly meant that every individual man is endowed with those faculties. And how many properties do these very philosophers boast of having demonstrated as belonging to substance in general, which is surely an idea as abstract as that of extension; and yet they maintain that all these properties apply to all individual substances, which are all extended. If, in effect, such a substance had not these properties, it would be false that they belonged to substance in general.

If then bodies, which infallibly are extended beings, or endowed with extension, were not divisible to infinity, it would be likewise false that divisibility in infinitum is a property of extension. Now those philosophers readily admit that this property belongs to extension, but they insist that it cannot take place in extended beings. This is the same thing with affirming that the understanding and will are indeed attributes of the notion of man in general, but that they can have no place in individual men actually existing.

Hence you will readily draw this conclusion: If divisibility in infinitum is a property of extension in general, it must of necessity likewise belong to all individual extended beings; or if real extended

beings are not divisible to infinity, it is false that divisibility in infinitum can be a property of extension in general.

It is impossible to deny the one or the other of these consequences without subverting the must solid principles of all knowledge; and the philosophers who refuse to admit divisibility in infinitum in real extended beings ought as little to admit it with respect to extension in general; but as they grant this last, they fall into a glaring contradiction.

You need not be surprised at this; it is a failing from which the greatest men are not exempt. But what is rather surprising, these philosophers, in order to get rid of their embarrassment, have thought proper to deny that body is extended. They say, that it is only an appearance of extension which is perceived in bodies, but that real extension by no means belongs to them.

You see clearly that this is merely a wretched cavil, by which the principal and the most evident property of body is denied. It is an extravagance similar to that formerly imputed to the Epicurean philosophers, who maintained that every thing which exists in the universe is material, without even excepting the gods, whose existence they admitted. But as they saw that these corporeal gods would be subjected to the greatest difficulties, they invented a subterfuge similar to that of our modern philosophers, alleging, that the gods had not bodies, but *as it were* bodies (*quasi corpora*), and that they had not senses, but senses *as it were;* and so of all the members. The other philosophical sects of antiquity made themselves abundantly merry with these *quasi corpora* and *quasi sensus;* and they would have equal reason in modern times to laugh at the *quasi extension* which our philosophers ascribe to body; this term *quasi extension* seems perfectly well to express that appearance of extension, without being so in reality.

Geometricians, if they meant to confound them, have only to say that the objects whose divisibility in infinitum they have demonstrated were likewise only *as it were* extended, and that accordingly all bodies extended *as it were* were necessarily divisible in infinitum. But nothing is to be gained with them; they resolve to maintain the greatest absurdities rather than acknowledge a mistake.

3d May, 1761.

LETTER X.

Of Monads.

When we talk in company on philosophical subjects, the conversation usually turns on such articles as have excited violent disputes among philosophers.

The divisibility of body is one of them, respecting which the sentiments of the learned are greatly divided. Some maintain that this divisibility goes on to infinity, without the possibility of ever arriving at particles so small as to be susceptible of no further division. But others insist that this division extends only to a certain point, and that you may come at length to particles so minute that, having no magnitude, they are no longer divisible. These ultimate particles, which enter into the composition of bodies, they denominate *simple beings* and *monads*.

There was a time when the dispute respecting monads employed such general attention, and was conducted with so much warmth, that it forced its way into company of every description, that of the guard-room not excepted. There was scarcely a lady at court who did not take a decided part in favour of monads or against them. In a word, all con-

versation was engrossed by monads—no other subject could find admission.

The Royal Academy of Berlin took up the controversy, and being accustomed annually to propose a question for discussion, and to bestow a gold medal, of the value of fifty ducats, on the person who, in the judgment of the Academy, has given the most ingenious solution, the question respecting monads was selected for the year 1748. A great variety of essays on the subject were accordingly produced. The president, *Mr. de Maupertuis*, named a committee to examine them, under the direction of the late *Count Dohna*, great chamberlain to the queen; who, being an impartial judge, examined with all imaginable attention the arguments adduced both for and against the existence of monads. Upon the whole, it was found that those which went to the establishment of their existence were so feeble and so chimerical, that they tended to the subversion of all the principles of human knowledge. The question was therefore determined in favour of the opposite opinion, and the prize adjudged to *Mr. Justi*, whose piece was deemed the most complete refutation of the monadists.

You may easily imagine how violently this decision of the Academy must have irritated the partisans of monads, at the head of whom stood the celebrated *Mr. Wolff*. His followers, who were then much more numerous and more formidable than at present, exclaimed in high terms against the partiality and injustice of the Academy; and their chief had wellnigh proceeded to launch the thunder of a philosophical anathema against it. I do not now recollect to whom we are indebted for the care of averting this disaster.

As this controversy has made a great deal of noise, you will not be displeased, undoubtedly, if I dwell a little upon it. The whole is reduced to this simple question, Is body divisible to infinity? or, in other words, Has the divisibility of bodies any bound, or has it not? I have already remarked as to this, that extension, geometrically considered, is on all hands allowed to be divisible in infinitum; because however small a magnitude may be, it is possible to conceive the half of it, and again the half of that half, and so on to infinity.

This notion of extension is very abstract, as are those of all *genera*, such as that of man, of horse, of tree, &c., as far as they are not applied to an individual and determinate being. Again, it is the most certain principle of all our knowledge, that whatever can be truly affirmed of the genus must be true of all the individuals comprehended under it. If therefore all bodies are extended, all the properties belonging to extension must belong to each body in particular. Now all bodies are extended, and extension is divisible to infinity; therefore every body must be so likewise. This is a syllogism of the best form; and as the first proposition is indubitable, all that remains is to be assured that the second is true, that is, whether it be true or not that bodies are extended.

The partisans of monads, in maintaining their opinion, are obliged to affirm that bodies are not extended, but have only an appearance of extension. They imagine that by this they have subverted the argument adduced in support of the divisibility in infinitum. But if body is not extended, I should be glad to know from whence we derived the idea of extension; for if body is not extended, nothing in the world is, as spirits are still less so: Our idea of extension, therefore, would be altogether imaginary and chimerical.

Geometry would accordingly be a speculation entirely useless and illusory, and never could admit of any application to things really existing. In effect, if no one thing is extended, to what purpose investigate the properties of extension? But as geometry

is beyond contradiction one of the most useful of the sciences, its object cannot possibly be a mere chimera.

There is a necessity then of admitting, that the object of geometry is at least the same apparent extension which those philosophers allow to body; but this very object is divisible to infinity: therefore existing beings endowed with this apparent extension must necessarily be extended.

Finally, let those philosophers turn themselves which way soever they will in support of their monads, or those ultimate and minute particles divested of all magnitude, of which, according to them, all bodies are composed, they still plunge into difficulties, out of which they cannot extricate themselves. They are right in saying that it is a proof of dulness to be incapable of relishing their sublime doctrine; it may however be remarked, that here the greatest stupidity is the most successful.

5th May, 1761.

LETTER XI.

Reflections on Divisibility in infinitum, and on Monads.

In speaking of the divisibility of body, we must carefully distinguish what is in our power, from what is possible in itself. In the first sense, it cannot be denied that such a division of body as we are capable of must be very limited.

By pounding a stone we can easily reduce it to powder; and if it were possible to reckon all the little grains which form that powder, their number would undoubtedly be so great, that it would be matter of surprise to have divided the stone into so many parts. But these very grains will be almost indivisible with respect to us, as no instrument we could employ will be able to lay hold of them. But it cannot with truth be affirmed that they are indivisible in themselves. You have only to view them with a good microscope, and each will appear itself a considerable stone, on which are distinguishable a great many points and inequalities; which demonstrates the possibility of a further division, though we are not in a condition to execute it. For wherever we can distinguish several points in any object, it must be divisible in so many parts.

We speak not, therefore, of a division practicable by our strength and skill, but of that which is possible in itself, and which the Divine Omnipotence is able to accomplish.

It is in this sense, accordingly, that philosophers use the word "divisibility:" so that if there were a stone so hard that no force could break it, it might be without hesitation affirmed that it is as divisible in its own nature as the most brittle of the same magnitude. And how many bodies are there on which we cannot lay any hold, and of whose divisibility we can entertain not the smallest doubt? No one doubts that the moon is a divisible body, though he is incapable of detaching the smallest particle from it: and the simple reason for its divisibility is its being extended.

Wherever we remark extension, we are under the necessity of acknowledging divisibility, so that divisibility is an inseparable property of extension. But experience likewise demonstrates that the division of bodies extends very far. I shall not insist at great length on the instance usually produced of a ducat: the artisan can beat it out into a leaf so fine as to cover a very large surface, and the ducat may be divided into as many parts as that surface is capable of being divided. Our own body furnishes an example much more surprising. Only consider the delicate veins and nerves with which it is filled, and the fluids which circulate through them. The subtilty there discoverable far surpasses imagination.

The smallest insects, such as are scarcely visible to the naked eye, have all their members, and legs on which they walk with amazing velocity. Hence we see that each limb has its muscles composed of a great number of fibres; that they have veins and nerves, and a fluid still much more subtile which flows through their whole extent.

On viewing with a good microscope a single drop of water, it has the appearance of a sea; we see thousands of living creatures swimming in it, each of which is necessarily composed of an infinite number of muscular and nervous fibres, whose marvellous structure ought to excite our admiration.* And though these creatures may perhaps be the smallest which we are capable of discovering by the help of the microscope, undoubtedly they are not the smallest which the Creator has produced. Animalcules probably exist as small relatively to them as they are relatively to us. And these after all are not yet the smallest, but may be followed by an infinity of new classes, each of which contains creatures incomparably smaller than those of the preceding class.

We ought in this to acknowledge the omnipotence and infinite wisdom of the Creator, as in objects of the greatest magnitude. It appears to me that the consideration of these minute species, each of which is followed by another inconceivably more minute, ought to make the liveliest impression on our minds, and inspire us with the most sublime ideas of the works of the Almighty, whose power knows no bounds, whether as to great objects or small.

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* A class of animals of superior magnitude, called medusæ, has been found so numerous as to discolour the ocean itself. Captain Scoresby found the number in the olive-green sea to be immense. A cubic inch contained 64, and consequently a cubic mile would contain 23,888,000,000 000,000. The same eminent navigator remarks, that if one person could count a million in *seven* days, it would have required that 80,000 persons should have started at the creation of the world to have completed the enumeration at the present time.—See Scoresby's *Account of the Arctic Regions*, and the article Phosphorescence, in the *Edinburgh Encyclopædia*, vol. xvi. p. 451.—*Ed.*

To imagine, that after having divided a body into a great number of parts, we arrive at length at particles so small as to defy all further division, is therefore the indication of a very contracted mind. But supposing it possible to descend to particles so minute as to be, in their own nature, no longer divisible, as in the case of the supposed monads; before coming to this point, we shall have a particle composed of only two monads, and this particle will be of a certain magnitude or extension, otherwise it could not have been divisible into these two monads. Let us further suppose that this particle, as it has some extension, may be the thousandth part of an inch, or still smaller if you will—for it is of no importance; what I say of the thousandth part of an inch may be said with equal truth of every smaller part. This thousandth part of an inch, then, is composed of two monads, and consequently two monads together would be the thousandth part of an inch, and two thousand times nothing a whole inch; the absurdity strikes at first sight.

The partisans of the system of monads accordingly shrink from the force of this argument, and are reduced to a terrible nonplus when asked how many monads are requisite to constitute an extension. Two, they apprehend, would appear insufficient, they therefore allow that more must be necessary. But if two monads cannot constitute extension, as each of the two has none, neither three, nor four, nor any number whatever will produce it; and this completely subverts the system of monads.

9th May, 1761.

LETTER XII.

Reply to the Objections of the Monadists to Divisibility in infinitum.

The partisans of monads are far from submitting to the arguments adduced to establish the divisibility of body to infinity. Without attacking them directly, they allege that divisibility in infinitum is a chimera of geometricians, and that it is involved in contradiction. For if each body is divisible to infinity, it would contain an infinite number of parts, the smallest bodies as well as the greatest; the number of these particles to which divisibility in infinitum would lead, that is to say, the most minute of which bodies are composed, will then be as great in the smallest body as in the largest, this number being *infinite* in both; and hence the partisans of monads triumph in their reasoning as invincible. For if the number of ultimate particles of which two bodies are composed is the same in both, it must follow, say they, that the bodies are perfectly equal to each other.

Now this goes on the supposition that the ultimate particles are all perfectly equal to each other; for if some were greater than others, it would not be surprising that one of the two bodies should be much greater than the other. But it is absolutely necessary, say they, that the ultimate particles of all bodies should be equal to each other, as they no longer have any extension, and their magnitude absolutely vanishes, or becomes nothing. They even form a new objection, by alleging that all bodies would be composed of an infinite number of nothings, which is a still greater absurdity.

I readily admit this; but I remark, at the same time, that it ill becomes them to raise such an objection, seeing they maintain that all bodies are composed of a certain number of monads, though, relatively to magnitude, they are absolutely nothings: so that by their own confession several nothings are capable of producing a body. They are right in saying their monads are not nothings, but beings endowed with an excellent quality, on which the nature of the bodies which they compose is founded. Now, the only question here is respecting extension; and as they are under the necessity of admitting that the monads have none, several nothings, according to them, would always be something.

But I shall push this argument against the system of monads no farther; my object being to make a direct reply to the objection founded on the ultimate particles of bodies, raised by the monadists in support of their system, by which they flatter themselves in the confidence of a complete victory over the partisans of divisibility in infinitum.

I should be glad to know, in the first place, what they mean by the *ultimate particles* of bodies. In their system, according to which every body is composed of a certain number of monads, I clearly comprehend that the ultimate particles of a body are the monads themselves which constitute it; but in the system of divisibility in infinitum, the term ultimate particle is absolutely unintelligible.

They are right in saying, that these are the particles at which we arrive from the division of bodies, after having continued it to infinity. But this is just the same thing with saying, after having finished a division which never comes to an end. For divisibility in infinitum means nothing else but the possibility of always carrying on the division, without ever arriving at the point where it would be necessary to stop. He who maintains divisibility in infinitum boldly denies, therefore, the existence of the ultimate particles of body; and it is a manifest contradiction to suppose at once ultimate particles and divisibility in infinitum.

I reply, then, to the partisans of the system of monads, that their objection to the divisibility of body to infinity would be a very solid one, did that system admit of ultimate particles; but being expressly excluded from it, all this reasoning of course falls to the ground.

It is false, therefore, that in the system of divisibility in infinitum bodies are composed of an infinity of particles. However closely connected these two propositions may appear to the partisans of monads, they manifestly contradict each other; for whoever maintains that body is divisible in infinitum, or without end, absolutely denies the existence of ultimate particles, and consequently has no concern in the question. The term can only mean such particles as are no longer divisible—an idea totally inconsistent with the system of divisibility in infinitum. This formidable attack, then, is completely repelled.

12*th May*. 1761.

LETTER XIII.

Principle of the Sufficient Reason, the strongest Support of the Monadists.

You must be perfectly sensible that one of the two systems which have undergone such ample discussion is necessarily true, and the other false, seeing they are contradictory.

It is admitted on both sides that bodies are divisible; the only question is, Whether this divisibility is limited? or, Whether it may always be carried further, without the possibility of ever arriving at indivisible particles?

The system of monads is established in the former case, since after having divided a body into indivisible particles, these very particles are monads, and there would be reason for saying that all bodies are composed of them, and each of a certain determinate number. Whoever denies the system of monads must likewise, then, deny that the divisibility of bodies is limited. He is under the necessity of maintaining that it is always possible to carry this divisibility further, without ever being obliged to stop; and this is the case of divisibility in infinitum, on which system we absolutely deny the existence of ultimate particles; consequently the difficulties resulting from their infinite number fall to the ground of themselves. In denying monads, it is impossible to talk any longer of ultimate particles, and still less of the number of them which enters into the composition of each body.

You must have remarked that what I have hitherto produced in support of the system of monads is destitute of solidity. I now proceed to inform you, that its supporters rest their cause chiefly on the great principle of the *sufficient reason*, which they know how to employ so dexterously that by means of it they are in a condition to demonstrate whatever suits their purpose, and to demolish whatever makes against them. The great discovery made, then, is this, *That nothing can be without a sufficient reason:* and to modern philosophers we stand indebted for it.

In order to give you an idea of this principle, you have only to consider, that in every thing presented to you, it may always be asked, Why is it such? And the answer is, what they call the *sufficient reason*, supposing it really to correspond with the question proposed. Wherever the *why* can take place, the possibility of a satisfactory answer is taken for granted, which shall, of course, contain the sufficient reason of the thing.

This is very far, however, from being a mystery of modern discovery. Men in every age have asked *why*—an incontestable proof of their conviction that

every thing must have a satisfying reason of its existence. This principle, that *nothing is without a cause*, was very well known to ancient philosophers; but unhappily this cause is for the most part concealed from us. To little purpose do we ask *why;* no one is qualified to assign the reason. It is not a matter of doubt that every thing has its cause; but a progress thus far hardly deserves the name; and so long as it remains concealed, we have not advanced a single step in real knowledge.

You may perhaps imagine that modern philosophers, who make such a boast of the principle of a sufficient reason, have actually discovered that of all things, and are in a condition to answer every *why* that can be proposed to them; which would undoubtedly be the very summit of human knowledge: but in this respect they are just as ignorant as their neighbours; their whole merit amounts to no more than a pretension to have demonstrated, that wherever it is possible to ask the question *why*, there must be a satisfactory answer to it, though concealed from us.

They readily admit that the ancients had a knowledge of this principle, but a knowledge very obscure; whereas they pretend to have placed it in its clearest light, and to have demonstrated the truth of it; and therefore it is that they know how to turn it most to their account, and that this principle puts them in a condition to prove that bodies are composed of monads.

Bodies, say they, must have their sufficient reason somewhere; but if they were divisible to infinity, such reason could not take place; and hence they conclude, with an air altogether philosophical, *that as every thing must have its sufficient reason, it is absolutely necessary that all bodies should be composed of monads*—which was to be demonstrated. This, I must admit, is a demonstration not to be resisted.

It were greatly to be wished that a reasoning so slight could elucidate to us questions of this importance; but I frankly confess I comprehend nothing of the matter. They talk of the sufficient reason of bodies, by which they mean to reply to a certain *wherefore*, which remains unexplained. But it would be proper, undoubtedly, clearly to understand and carefully to examine a question, before a reply is attempted; in the present case, the answer is given before the question is formed.

Is it asked, Why do bodies exist? It would be ridiculous, in my opinion, to reply, Because they are composed of monads; as if they contained the cause of that existence. Monads have not created bodies; and when I ask, Why such a being exists? I see no other reason that can be given but this, Because the Creator has given it existence; and as to the manner in which creation is performed, philosophers, I think, would do well honestly to acknowledge their ignorance.

But they maintain, that God could not have produced bodies without having created monads, which were necessary to form the composition of them. This manifestly supposes that bodies are composed of monads, the point which they meant to prove by this reasoning. And you are abundantly sensible, that it is not fair reasoning to take for granted the truth of a proposition which you are bound to prove by reasoning. It is a sophism known in logic by the name of a *petitio principii*, or begging the question.

16*th May*, 1761.

LETTER XIV.

Another Argument of the Monadists, derived from the Principle of the Sufficient Reason. Absurdities resulting from it.

The partisans of monads likewise derive their grand argument from the principle of the sufficient reason, by alleging that they could not even comprehend the possibility of bodies, if they were divisible to infinity, as there would be nothing in them capable of checking imagination; they must have ultimate particles or elements, the composition of which must serve to explain the composition of bodies.

But do they pretend to understand the possibility of all the things which exist? This would savour too much of pride; nothing is more common among philosophers than this kind of reasoning—I cannot comprehend the possibility of this, unless it is such as I imagine it to be: therefore it necessarily must be such.

You clearly comprehend the frivolousness of such reasoning; and that in order to arrive at truth, research much more profound must be employed. Ignorance can never become an argument to conduct us to the knowledge of truth, and the one in question is evidently founded on ignorance of the different manners which may render the thing possible.

But on the supposition that nothing exists but that whose possibility they are able to comprehend, is it possible for them to explain how bodies would be composed of monads? Monads, having no extension, must be considered as points in geometry, or as we represent to ourselves spirits and souls. Now it is well known that many geometrical points, let the number be supposed ever so great, never can produce a line, and consequently still less a surface, or a body. If a thousand points were sufficient to constitute the thousandth part of an inch, each of these must necessarily have an extension, which taken a thousand times would become equal to the thousandth part of an inch. Finally, it is an incontestable truth, that take any number of points you will, they can never produce extension. I speak here of points such as we conceive in geometry, without any length, breadth, or thickness, and which in that respect are absolutely nothing.

Our philosophers accordingly admit that no extension can be produced by geometrical points, and they solemnly protest that their monads ought not to be confounded with these points. They have no more extension than points, say they; but they are invested with admirable qualities, such as representing to them the whole universe by ideas, though extremely obscure; and these qualities render them proper to produce the phenomenon of extension, or rather that apparent extension which I formerly mentioned. The same idea, then, ought to be formed of monads as of spirits and souls, with this difference, that the faculties of monads are much more imperfect.

The difficulty appears to me by this greatly increased; and I flatter myself you will be of my opinion that two or more spirits cannot possibly be joined so as to form extension. Several spirits may very well form an assembly or a council, but never an extension; abstraction made of the body of each counsellor, which contributes nothing to the deliberation going forward, for this is the production of spirits only; a council is nothing else but an assembly of spirits or souls: but could such an assembly represent an extension? Hence it follows that monads are still less proper to produce extension than geometrical points are.

The partisans of the system, accordingly, are not agreed as to this point. Some allege, that monads are actual parts of bodies; and that after having divided a body as far as possible, you then arrive at the monads which constitute it.

Others absolutely deny that monads can be considered as constituent parts of bodies; according to them, they contain only the sufficient reason: while the body is in motion, the monads do not stir, but they contain the sufficient reason of motion. Finally, they cannot touch each other; thus, when my hand touches a body, no one monad of my hand touches a monad of the body.

What is it then, you will ask, that touches in this case, if it is not the monads which compose the hand and the body? The answer must be, that two nothings touch each other, or rather it must be denied that there is a real contact. It is a mere illusion, destitute of all foundation. They are under the necessity of affirming the same thing of all bodies, which, according to these philosophers, are only phantoms formed by the imagination, representing to itself very confusedly the monads which contain the sufficient reason of all that we denominate body.

In this philosophy every thing is spirit, phantom, and illusion; and when we cannot comprehend these mysteries, it is our stupidity that keeps up an attachment to the gross notions of the vulgar.

The greatest singularity in the case is, that these philosophers, with a design to investigate and explain the nature of bodies and of extension, are at last reduced to deny their existence. This is undoubtedly the surest way to succeed in explaining the phenomena of nature; you have only to deny them, and to allege in proof the principle of the sufficient reason. Into such extravagances will philosophers run rather than acknowledge their ignorance.

19*th May*, 1761.

LETTER XV.

Reflections on the System of Monads.

It would be a great pity, however, that this ingenious system of monads should crumble into ruins. It has made too much noise, it has cost its partisans too many sublime and profound speculations, to be permitted to sink into total oblivion. It will ever remain a striking monument of the extravagance into which the spirit of philosophizing may run. It is well worth while, then, to present you with a more particular account of it.

It is necessary, first of all, to banish from the mind every thing corporeal—all extension, all motion, all time and space—for all these are mere illusion. Nothing exists in the world but monads, the number of which undoubtedly is prodigious. No one monad is to be found in connexion with others; and it is demonstrated by the principle of the sufficient reason that monads can in no manner whatever act upon each other. They are indeed invested with powers, but these are exerted only within themselves, without having the least influence externally.

These powers, with which each monad is endowed, have a tendency only to be continually changing their own state, and consist in the representation of all other monads. My soul, for example, is a monad, and contains in itself ideas of the state of all other monads. These ideas are for the most part very obscure; but the powers of my soul are continually employed in their further elucidation, and in carrying them to a higher degree of clearness. Other monads have, in this respect, a sufficient resemblance to my soul; each is replete with a prodigious quantity of obscure ideas of all other monads, and of their state; and they are continually exerting

themselves with more or less success in unfolding these ideas, and in carrying them to a higher degree of clearness.

Such monads as have succeeded better than I have done are spirits more perfect; but the greater part still remain in a state of stagnation, in the greatest obscurity of their ideas; and when they are the object of the ideas of my soul, they produce in it the illusory and chimerical idea of extension and of body. As often as my soul thinks of bodies and of motion, this proves that a great quantity of other monads are still buried in their obscurity; it is likewise when I think of them that my soul forms within itself the idea of some extension, which is consequently nothing but mere illusion.

The more monads there are plunged in the abyss of the obscurity of their ideas, the more is my soul dazzled with the idea of extension; but when they come to clear up their obscure ideas, extension seems to me to diminish, and this produces in my soul the illusory idea of motion.

You will ask, no doubt, how my soul perceives that other monads succeed in developing their obscure ideas, seeing there is no connexion between them and me. The partisans of the system of monads are ready with this reply, that it takes place conformably to the perfect harmony which the Creator (who is himself only a monad) has established between monads, by which each perceives in itself, as in a mirror, every development produced in others, without any manner of connexion between them.

It is to be hoped, then, that all monads may at length become so happy as to clear up their obscure ideas, and then we should lose all ideas of body and of motion; and the illusion, arising merely from the obscurity of ideas, would entirely cease.

But there is little appearance of the arrival of this blessed state; most monads, after having acquired the capacity of clearing up their obscure ideas, suddenly relapse. When shut up in my chamber, I perceive myself but of small extension, because several monads have then unfolded their ideas; but as soon as I walk abroad, and contemplate the vast expanse of heaven, they must all have relapsed into their state of dulness.

There is no change of place or of motion; all that is illusion merely: my soul remains almost always in the same place, just as all other monads. But when it begins to unfold some ideas which before were but very obscure, it appears to me then that I am approaching the object which they represent to me, or rather that which the monads of such idea excite in me; and this is the real explanation of the phenomenon, when it appears to us that we are approaching to certain objects.

It happens but too frequently that the elucidations we had acquired are again lost; then it appears to us that we are removing from the same object. And here we must look for the true solution of our journeyings. My idea, for example, of the city of Magdeburg is produced by certain monads, of which at present I have but very obscure ideas; and this is the reason why I consider myself as at a distance from Magdeburg. Last year these same ideas suddenly became clear, and then I imagined I was travelling to Magdeburg, and that I remained there several days. This journey, however, was an illusion merely, for my soul never stirs from its place. It is likewise an illusion when you imagine yourself absent from Berlin, because the confused representation of certain monads excites an obscure idea of Berlin, which you have only to clear up, and that instant you are at Berlin. Nothing more is necessary. What we call journeys, and on which we expend so much money, is mere illusion. Such is the real plan of the system of monads.

You will ask, Is it possible there ever should have been persons of good sense who seriously maintained

these extravagances? I reply, there have been but too many, that I know several of them, that there are some at Berlin, nay, perhaps at Magdeburg.

23d May, 1761.

LETTER XVI.

Continuation.

The system of monads, such as I have been describing it, is a necessary consequence from the principle that bodies are compounded of simple beings. The moment this principle is admitted, you are obliged to acknowledge the justness of all the other consequences, which result from it so naturally that it is impossible to reject any one, however absurd and contradictory.

First, these simple beings, which must enter into the composition of bodies, being monads which have no extension, neither can their compounds, that is bodies, have any; and all these extensions become illusion and chimera, it being certain that parts destitute of extension are incapable of producing a real extension; it can be at most an appearance or a phantom, which dazzles by a fallacious idea of extension. In a word, every thing becomes illusion; and upon this is founded the system of pre-established harmony, the difficulties of which I have already pointed out.

It is necessary then to take care that we be not entangled in this labyrinth of absurdities. If you make a single false step over the threshold, you are involved beyond the power of escaping. Every thing depends on the first ideas formed of extension: and the manner in which the partisans of the system of monads endeavour to establish it is extremely seductive.

These philosophers do not like to speak of the extension of bodies, because they clearly foresee that it must become fatal to them in the sequel; but instead of saying that bodies are extended, they denominate them compound beings, which no one can deny, as extension necessarily supposes divisibility, and consequently a combination of parts which constitute bodies. But they presently make a wrong use of this notion of a compound being. For, say they, a being can be compounded only so far as it is made up of simple beings; and hence they conclude that every body is compounded of simple beings. As soon as you grant them this conclusion, you are caught beyond the power of retreating; for you are under the necessity of admitting that these simple beings, not being compounded, are not extended.

This captious argument is exceedingly seductive. If you permit yourself to be dazzled with it, they have gained their point. Only admit this proposition, bodies are compounded of simple beings, that is, of parts which have no extension, and you are entangled. With all your might, then, resist this assertion—*every compound being is made up of simple beings;* and though you may not be able directly to prove the fallacy, the absurd consequences which immediately result would be sufficient to overthrow it.

In effect, they admit that bodies are extended; from this point the partisans of the system of monads set out to establish the proposition that they are compound beings; and having hence deduced that bodies are compounded of simple beings, they are obliged to allow that simple beings are incapable of producing real extension, and consequently that the extension of bodies is mere illusion.

An argument whose conclusion is a direct contradiction of the premises is singularly strange: this reasoning sets out with advancing that bodies are extended; for if they were not, how could it be known that they are compound beings—and then comes the conclusion that they are not so. Never was a fallacious argument, in my opinion, more completely refuted than this has been. The question was, *Why are bodies extended?* And, after a little turning and winding, it is answered, *Because they are not so.* Were I to be asked, Why has a triangle three sides? and I should reply that it is a mere illusion—would such a reply be deemed satisfactory?

It is therefore certain that this proposition, "Every compound being is necessarily made up of simple beings," leads to a false conclusion, however well founded it may appear to the partisans of monads, who even pretend to rank it among the axioms or first principles of human knowledge. The absurdity in which it immediately issues is sufficient to overturn it, were there no other reasons for calling it in question.

But as a compound being here means the same thing as an extended being, it is just as if it were affirmed, "Every extended being is compounded of beings which are not so." And this is precisely the question. It is asked, Whether on dividing a body you arrive at length at parts unsusceptible of any further division, for want of extension; or, Whether you never arrive at particles such as that the divisibility should be unbounded?

In order to determine this important question, for the sake of argument let it be supposed that every body is compounded of parts without extension. Certain specious reasonings may easily be employed, drawn from the noted principle of the sufficient reason; and it will be said that a compound being can have its sufficient reason only in the simple beings which compose it; which might be true if the compound being were in fact made up of simple beings, the very point in question; and whenever this composition is denied, the sufficient reason becomes totally inapplicable.

But it is dangerous to enter the lists with persons who believe in monads; for, besides that there is nothing to be gained, they loudly exclaim that you are attacking the principle of the sufficient reason, which is the basis of all certainty, even of the existence of God. According to them, whoever refuses to admit monads, and rejects the magnificent fabric, in which every thing is illusion, is an infidel and an atheist. Sure I am that such a frivolous imputation will not make the slightest impression on your mind, but that you will perceive the wild extravagances into which men are driven when they embrace the system of monads—a system too absurd to need a refutation in detail; their foundation being absolutely reduced to a wretched abuse of the principle of the sufficient reason.

26th May, 1761.

LETTER XVII.

Conclusion of Reflections on this System.

We are under the necessity of acknowledging the divisibility of bodies in infinitum, or of admitting the system of monads, with all the extravagances resulting from it; there is no other choice—an alternative which supplies the partisans of that system with another formidable argument in support of it.

They pretend, that by divisibility in infinitum we are obliged to ascribe to bodies an infinite quality, whereas it is certain that God alone is infinite.

The partisans of the system of monads are very dangerous persons; they accused us of atheism, and now they charge us with polytheism, alleging that we ascribe to all bodies infinite perfections. Thus we should be much worse than pagans, who only worship certain idols, whereas we are accused of paying homage to all bodies, as so many divinities. A dreadful imputation, no doubt, were it well founded; and I should certainly prefer embracing the system of monads, with all the chimeras and illusions which flow from it, to a declaration in favour of divisibility in infinitum, if it involved a conclusion so impious.

You will allow, that to reproach one's adversaries with atheism or idolatry is a very strange mode of arguing; but where do they find us ascribing to bodies this divine infinity? Are they infinitely powerful, wise, good, or happy? By no means: we only affirm, that on dividing bodies, though the division be carried on ever so far, it will always be possible to continue it further, and that you never can arrive at indivisible particles. It may accordingly be affirmed, that the divisibility of bodies is without *limits;* and it is improper to use the term *infinity*, which is applicable to God alone.

I must remark at the same time, that the word "infinity" is not so dangerous as these philosophers insinuate. In saying, for example, infinitely wicked, nothing is more remote from the perfections of God.

They admit that our souls will never have an end, and thus acknowledge an infinity in the duration of the soul, without marking the least disrespect to the infinite perfections of God. Again, when you ask them if the extent of the universe is bounded, are they very indecisive in their answer? Some of them very frankly allow that the extent of the universe may very probably be infinite without our being able, however far our ideas are carried, to determine its limits. Here then is one infinity more which they do not deem heretical.

For a still stronger reason, divisibility in infinitum ought not to give them the least offence. To be divisible to infinity is not surely an attribute which any one could ever think of ascribing to the Supreme Being, and does not confer on bodies a degree of perfection which would not be far from that which these philosophers allow them in compounding them of monads, which on their system are beings endowed with qualities so eminent that they do not hesitate to give to God himself the denomination of monad.

In truth, the idea of a division which may be continued without any bounds contains so little of the character of the Deity that it rather places bodies in a rank far inferior to that which spirits and our souls occupy; for it may well be affirmed that a soul in its essence is infinitely more valuable than all the bodies in the world. But on the system of monads, every body, even the vilest, is compounded of a vast number of monads, whose nature has a great resemblance to that of our souls. Each monad represents to itself the whole world as easily as our souls; but, say they, their ideas of it are very obscure, though we have already clear, and sometimes also distinct ideas of it.

But what assurance have they of this difference? Is it not to be apprehended that the monads which compose the pen wherewith I am writing may have ideas of the universe much clearer than those of my soul? How can I be assured of the contrary? I ought to be ashamed to employ a pen in conveying my feeble conceptions, while the monads of which it consists possibly conceive much more sublimely; and you might have greater reason to be satisfied, should the pen commit its own thoughts to paper instead of mine.

In the system of monads that is not necessary; the soul represents to itself beforehand, by its inherent powers, all the ideas of my pen, but in a very obscure manner. What I am now taking the liberty to suggest contributes absolutely nothing to your information. The partisans of this system have demonstrated that simple beings cannot exercise the slightest influence on each other; and your own soul derives from itself what I have been endeavouring to convey, without my having any concern in the matter.

Conversation, reading, and writing, therefore, are merely chimerical and deceptive formalities, which illusion would impose upon us as the means of acquiring and extending knowledge. But I have already had the honour of pointing out to you the wonderful consequences resulting from the system of the pre-established harmony; and I am apprehensive that these reveries may have become too severe a trial of your patience, though many persons of superior illumination consider this system as the most sublime production of human understanding, and are incapable of mentioning it but with the most profound respect.[*]

30*th May*, 1761.

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[*] It is a consolation to reflect, that philosophy has in modern times divested itself of the lumber of such idle disputations as those of which our author has in the preceding Letters of this, and in several of the former, volume given us so full an account. The disputes about pre-established harmonies, and the nature and existence of monads, and of the essences of things appear to have been owing to the want of a just conception of the limitation which Divine Providence has assigned to the powers and faculties of the human mind. Infinity, whether in the great or in the small, is absolutely beyond our reach. That nature carries the division of matter both in the organic and inorganic world (as the microscope reveals to us in the astonishing minuteness of animalculæ, and as the sense of smelling determines in the diffusion of odours) to an extent beyond our comprehension as to the means employed, no one can doubt; but with respect to the question of infinite divisibility, about which so much has been said, although in the abstract it may seem to be established in the affirmative by geometrical reasoning, yet it is the prevailing opinion of the present day that there is a limitation in nature of actual divisibility. The atoms or elementary particles of the chymist appear to furnish the ultimatum of the process of nature in the divisibility of matter. That different kinds of matter are constituted of different sorts of simple or elementary atoms, having different qualities or affinities, and that these atoms possess infinite hardness, and cannot therefore be further divided, are propositions which enable us to account more satisfactorily for the chymical changes which are constantly taking place throughout the whole domain of nature, and for the stability of the laws to which those changes are subservient. We know, indeed, little or nothing of the real nature of corpuscular action, but the theory of atomic combinations in stable and definite proportions has diffused a most salutary light over the whole surface of chymical science. It will be a gratification to every Christian reader to observe the ability with which Euler combats the skeptical philosophy which resulted from the visionary theories which he has so ably confuted.—*Am. Ed.*

LETTER XVIII.

Elucidation respecting the Nature of Colours.

I AM under the necessity of acknowledging, that the ideas respecting colour which I have already taken the liberty to suggest* come far short of that degree of evidence to which I could have wished to carry them. This subject has hitherto proved a stumbling-block to philosophers, and I must not flatter myself with the belief that I am able to clear it of every difficulty. I hope, at the same time, that the elucidations which I am going to submit to your examination may go far towards removing a considerable part of them.

The ancient philosophers ranked colours among the bodies of which we know only the names. When they were asked, for example, why such a body was red, they answered, it was in virtue of a quality which made it appear red. You must be sensible that such an answer conveys no information, and that it would have been quite as much to the purpose to confess ignorance.

Descartes, who first had the courage to plunge into the mysteries of nature, ascribes colours to a certain mixture of light and shade, which last, being nothing else but a want of light, as it is always found where the light does not penetrate, must be incapable of producing the different colours we observe.

Having remarked that the sensations of the organ of sight are produced by the rays which strike that organ, it necessarily follows that those which excite in it the sensation of red must be of quite a different nature from those which produce the sensation of the other colours; hence it is easily comprehended that each colour is attached to a certain quality of the rays which strike the organ of vision. A body appears to us red when the rays which it emits are of a nature to excite in our eyes the sensation of that colour.

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* See Letters XXVII., XXVIII., and XXXI., in vol. i.

The whole, then, results in an inquiry into the difference of the rays which variety of colours produces. This difference must be great to produce so many particular sensations in our eyes. But wherein can it consist? This is the great question, towards the solution of which our present research is directed.

The first difference between rays which presents itself is that some are stronger than others. It cannot be doubted that those of the sun, or of any other body very brilliant, or very powerfully illuminated, must be much stronger than those of a body feebly illuminated, or endowed with a slender degree of light; our eyes are assuredly struck in a very different manner by the one and by the other.

Hence it might be inferred, that different colours result from the force of the rays of light; so that the most powerful rays should produce, for example, red; those which are less so, yellow; and in progression, green, and blue.

But there is nothing more easy than to overturn this system, as we know from experience that the same body always appears to be of the same colour, be it less or more illuminated, or whether its rays be strong or feeble. A red body, for example, appears equally red, exposed to the brightest lustre of the sun, and in the shade, where the rays are extremely faint. We must not, then, look for the cause of the difference of colour in the different degrees of the force of rays of light, it being possible to represent the same colour as well by very forcible as by very faint rays. The feeblest glimmering serves equally well to discover to us difference of colours, as the brightest effulgence.

It is absolutely necessary, therefore, that there should be another difference of rays discovered, which may characterize their nature relatively to the different colours. You will undoubtedly conclude, that in order to discover this difference we must be better acquainted with the nature of luminous rays; in other words, we must know what it is that, reaching our eyes, renders bodies visible; this definition of a ray must be the justest, as in effect it is nothing else but that which enters into the eye by the pupil, and excites the sensation in it.

I have already informed you that there are only two systems or theories which pretend to explain the origin and nature of rays of light. The one is that of *Newton*, who considers them as emanations proceeding from the sun and other luminous bodies; and the other that which I have endeavoured to demonstrate, and of which I have the reputation of being the author, though others have had nearly the same ideas of it. Perhaps I may have succeeded better than they in carrying it to a higher degree of evidence. It will be of importance, then, to show, in both systems, on what principle the difference of colours may be established.

In that of emanation, which supposes the rays to issue from luminous bodies, in the form of rivers, or rather of fountains, spouting out a fluid in all directions, it is alleged that the particles of light differ in size or in substance, as a fountain might emit wine, oil, and other liquids; so that the different colours are occasioned by the diversity of the subtile matter which emanates from luminous bodies. Red would be, accordingly, a subtile matter issuing from the luminous body, and so of yellow and the other colours. This explanation would exhibit clearly enough the origin of the different colours, if the system itself had a solid foundation. I shall enter into the subject more at large in my next Letter.

2d June, 1761.

LETTER XIX.

Reflections on the Analogy between Colours and Sounds.

You will be pleased to recollect the objections I offered to the system of the emanation of light.* They appear to me so powerful as completely to overturn that system. I have accordingly succeeded in my endeavours to convince certain natural philosophers of distinction, and they have embraced my sentiments of the subject with expressions of singular satisfaction.

Rays of light, then, are not an emanation from the sun and other luminous bodies, and do not consist of a subtile matter emitted forcibly by the sun, and transmitted to us with a rapidity which may well fill you with astonishment. If the rays employed only eight minutes in their course from the sun to us, the torrent would be terrible,† and the mass of that luminary, however vast, must speedily be exhausted.

According to my system, the rays of the sun, of which we have a sensible perception, do not proceed immediately from that luminary; they are only particles of ether floating around us, to which the sun communicates nearer and nearer a motion of vibration, and consequently they do not greatly change their place in this motion.

This propagation of light is performed in a manner similar to that of sound. A bell, whose sound you hear, by no means emits the particles which enter your ears. You have only to touch it when struck to be assured that all its parts are in a very sensible agitation. This agitation immediately communicates itself to the more remote particles of air, so that all receive from it successively a similar motion of vibration, which, reaching the ear, excite in it the sensation of sound. The strings of a musical instrument put the matter beyond all doubt; you see them tremble, go and come. It is even possible to detertermine by calculation how often in a second each string vibrates; and this agitation, being communicated to the particles of air adjacent to the organ of hearing, the ear is struck by it precisely as often in a second. It is the perception of this tremulous agitation which constitutes the nature of sound. The greater the number of vibrations produced by the string in a second, the higher or sharper is the sound. Vibrations less frequent produce lower notes.

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* See Letters XVII. and XVIII. in vol. i.

† The rapidity of the progress of light, if it be objectionable on the theory of emanation, must be equally so on that of undulation; for the velocity is a fact derived from observation, and is independent of theory.—*Am. Ed.*

We find the circumstances which accompany the sensation of hearing, in a manner perfectly analogous, in that of sight.

The medium only and the rapidity of the vibrations differ. In sound, it is the air through which the vibrations of sonorous bodies are transmitted. But with respect to light, it is the ether, or that medium incomparably more subtile and more elastic than air, which is universally diffused wherever the air and grosser bodies leave interstices.

As often, then, as this ether is put into a state of vibration, and is transmitted to the eye, it excites in it the sentiment of vision, which is in that case nothing but a similar tremulous motion, whereby the small nervous fibres at the bottom of the eye are agitated.

You easily comprehend that the sensation must be different, according as this tremulous agitation is more or less frequent; or according as the number of vibrations performed in a second is greater or less. Hence there must result a difference similar to that which takes place in sounds, when the vibrations are more or less frequent. This difference is clearly perceptible by the ear, as the character of sounds in respect of flat and sharp depends on it. You will recollect that the note marked C in the harpsichord performs about 100 vibrations in a second, note D 112, note E 125, note F 133, note G 150, note A 166, note B 187, and 200. Thus the nature of sounds depends on the number of vibrations performed in a second.

It cannot be doubted that the sense of seeing may be likewise differently affected, according as the number of vibrations of the nervous fibres of the bottom of the eye is greater or less. When these fibres vibrate 1000 times in a second, the sensation must be quite different from what it would be did they vibrate 1200 or 1500 times in the same space.

True it is that the organ of vision is not in a condition to reckon numbers so great, still less than the ear is to reckon the vibrations which constitute sound; but it is always in our power to distinguish between the greater and the less.

In this difference, therefore, we must look for the cause of difference of colour; and it is certain that each of them corresponds to a certain number of vibrations, by which the fibres of our eyes are struck in a second, though we are not as yet in a condition to determine the number corresponding to each particular colour, as we can do with respect to sounds.

Much research must have been employed before it was possible to ascertain the numbers corresponding to all the notes of the harpsichord, though there was an antecedent conviction that their difference was founded on the diversity of those numbers. Our knowledge respecting these objects is nevertheless considerably advanced, from our being assured that there prevails a harmony so delightful between the different notes of the harpsichord and the different colours; and that the circumstances of the one serve to elucidate those of the other. This analogy accordingly furnishes the most convincing proofs in support of my system. But I have reasons still more solid to adduce, which will secure it from every attack.

6th June, 1761.

LETTER XX.

Continuation.

NOTHING is more adapted to the communication of knowledge respecting the nature of vision than the analogy discoverable, almost in every particular, between it and the hearing. Colours are to the eye what sounds are to the ear. They differ from each other as flat and sharp notes differ. Now we know that flat and sharp in sounds depends on the number of vibrations whereby the organ of hearing is struck in a given time, and that the nature of each is determined by a certain number, which marks the vibrations performed in a second. From this I conclude that each colour is likewise restricted to a number of vibrations which act on vision; with this difference, that the vibrations which produce sound reside in gross air, whereas those of light and colours are transmitted through a medium incomparably more subtile and elastic. The same thing holds as to the objects of both senses. Those of hearing are all of them bodies adapted to the transmission of sound, that is, susceptible of a motion of vibration, or of a tremulous agitation, which, communicating itself to the air, excites in the organ the sensation of a sound corresponding to the rapidity of the vibrations.

Such are all musical instruments; and to confine myself principally to the harpsichord, we ascribe to each string a certain sound which it produces when struck. Thus, one string is named C, another D, and so on. A string is named C when its structure

and tension are such that, being struck, it produces about 100 vibrations in a second; and if it produced less or more in the same time, it would have the name of a different note, higher or lower.

You will please to recollect that the sound of a string depends on three things—its length, its thickness, and the degree of tension; the more it is stretched the sharper its sound becomes; and as long as it preserves the same disposition, it emits the same sound; but that changes as soon as the other undergoes any variation.

Let us apply this to bodies which are the objects of vision. The minuter particles which compose the tissue of their surface may be considered as strings distended, in as much as they are endowed with a certain degree of elasticity and bulk, so that, being struck, they acquire a motion of vibration, of which they will finish a certain number in a second; and on this number depends the colour which we ascribe to such body. It is red when the particles of its surface have such a degree of tension that, being agitated, they perform precisely so many vibrations in a second as are necessary to excite in us the sensation of that colour. A degree of tension which would produce vibrations more or less rapid would excite that of a different colour, and then the body would be yellow, green, or blue, &c.

We have not as yet acquired the ability of assigning to each colour the number of vibrations which constitute its essence; we do not so much as know which are the colours that require a greater or less rapidity of vibration, or rather, it is not yet determined what colours correspond with high or low notes. It is sufficient to know that each colour is attached to a certain number of vibrations, though it has not hitherto been ascertained; and that you have only to change the tension of elasticity of the particles which form the surface of a body, to make it change colour.

We see that the most beautiful colours in flowers quickly change and disappear, from a failure of the nutritive juices; and because their particles lose their vigour or their tension. This, too, is observable in every other change of colour.

To place this in a clearer light, let us suppose that the sensation of red requires such a rapidity of vibration, that 1000 are performed in a second; that orange requires 1125, yellow 1250, green 1333, blue 1500, and violet 1666. Though these numbers are only supposed, this does not affect the object I have in view. What I say as to these numbers will apply in like manner to the really corresponding numbers, if ever they are discovered.

A body, then, will be red when the particles of its surface, put in vibration, complete 1000 in a second; another body will be orange when disposed so as to complete 1125 in a second, and so on. Hence it is obvious that there must be an endless variety of intermediate colours between the six principal which I have mentioned; and it is likewise evident, if the particles of a body, being agitated, should perform 1400 vibrations in a second, it would be of an intermediate colour between green and blue; green corresponding to number 1333, and blue to 1500.

9th June, 1761.

LETTER XXI.

How Opaque Bodies are rendered visible.

You will find no difficulty in the definition I have been giving of coloured bodies. The particles of their surface are always endowed with a certain degree of elasticity, which renders them susceptible of a motion of vibration, as a string is always susceptible of a certain sound; and it is the number of vibrations which these particles are capable of

making in a second which determines the species of colour.

If the particles of the surface have not elasticity sufficient to admit of such agitation, the body must be black, this colour being nothing else but a deprivation of light, and all bodies from which no rays are transmitted to our eyes appearing black.

I now come to a very important question, respecting which some doubts may be entertained. It may be asked, What is the cause of the motion of vibration which constitutes the colours of bodies?

Into the discovery of this, indeed, the whole is resolved; for as soon as the particles of bodies shall be put in motion, the ether diffused through the air will immediately receive a similar agitation, which, continued to our eyes, constitutes there that which we call *rays*, from which vision proceeds.

I remark, first, that the particles of bodies are not put in motion by an internal, but an external power, just as a string distended would remain for ever at rest, were it not put in motion by some external force. Such is the case of all bodies in the dark; for, as we see them not, it is a certain proof that they emit no rays, and that their particles are at rest. In other words, during the night bodies are in the same state with the strings of an instrument that is not touched, and which emit no sound; whereas bodies rendered visible may be compared to strings which emit sound.

And as bodies become visible as soon as they are illuminated, that is, as soon as the rays of the sun, or of some other luminous body, fall upon them, it must follow, that the same cause which illuminates them must excite their particles to generate rays, and to produce in our eyes the sensation of vision. The rays of light, then, falling upon a body, put its particles into a state of vibration.

This appears at first surprising, because on exposing our hands to the strongest light no sensible impression is made on them. It is to be considered, that the sense of touch is in us too gross to perceive these subtile and slight impressions; but that the sense of sight, incomparably more delicate, is powerfully affected by them. This furnishes an incontestable proof that the rays of light which fall upon a body possess sufficient force to act upon the minuter particles, and to communicate to them a tremulous agitation. And in this precisely consists the action necessary to explain how bodies, when illuminated, are put in a condition themselves to produce rays, by means of which they become visible to us. It is sufficient that bodies should be luminous or exposed to the light, in order to the agitation of their particles, and thereby to their producing themselves rays which render them visible to us.

The perfect analogy between hearing and sight gives to this explanation the highest degree of probability. Let a harpsichord be exposed to a great noise, and you will see that not only the strings in general are put into a state of vibration, but you will hear the sound of each, almost as if it were actually touched. The mechanism of this phenomenon is easily comprehended, as soon as it is known that a string agitated is capable of communicating to the air the same motion of vibration which, transmitted to the ear, excites in it the sensation of the sound which that same string emits.

Now, as a string produces in the air such a motion, it follows that the air reciprocally acts on the string, and gives it a tremulous motion. And as a noise is capable of putting in motion the strings of a harpsichord, and of extracting sounds from them, the same thing must take place in the objects of vision.

Coloured bodies are similar to the strings of a harpsichord, and the different colours to the different notes, in respect of high and low. The light which falls on these bodies, being analogous to the noise to which the harpsichord is exposed, acts on the particles of their surface as that noise acts on the strings of the harpsichord; and these particles thus put in vibration will produce the rays which shall render the body visible.

This elucidation seems to me sufficient to dissipate every doubt relating to my theory of colours. I flatter myself, at least, that I have established the true principle of all colours, as well as explained how they become visible to us only by the light whereby bodies are illuminated, unless such doubts turn upon some other point which I have not touched upon.

13th June, 1761.

LETTER XXII.

The Wonders of the Human Voice.

In explaining the theory of sounds, I considered only two respects in which sounds could differ: the one regarded the force of sound, and I remarked that it is greater in proportion as the vibrations excited in the air are more violent. Thus, the noise of a discharge of cannon, or the ringing of a bell, has more force than that of a string, or of the human voice.

The other difference of sounds is totally independent of this, and refers to flat and sharp, according to which we say some are low and others high. My remark relatively to this difference made it to depend on the number of vibrations completed in a certain given time, say a second; so that the greater such number is, the higher or sharper is the sound; and the smaller it is, the sound is lower or flatter.

You can easily comprehend how the same note may be either strong or faint; accordingly, we see that the *forte* and *piano* employed by musicians change in no respect the nature of sounds. Among the good qualities of a harpsichord, it is required that all the notes should have nearly the same degree of strength; and it is always considered as a great fault when some of the strings are wound up to a greater degree of force than the rest. Now the flat and the sharp are referable only to the simple sounds, whose vibrations follow regularly, and at equal intervals; and in music we employ only those sounds which are denominated simple. Accords are compound sounds, or the concourse of several produced at once, among the vibrations of which a certain order must predominate, which is the foundation of harmony. But when no relation among the vibrations is perceptible, it is a confused noise, with which it is impossible to say what note of the harpsichord is in tune, such as the report of a cannon or musket.

There is still another remarkable difference among the simple sounds, which seems to have escaped the attention of philosophers. Two sounds may be of equal force, and in accord with the same note of the harpsichord, and yet very different to the ear. The sound of a flute is totally different from that of the French horn, though both may be in tune with the same note of the harpsichord, and equally strong; each sound derives a certain peculiarity from the instrument which emits it, but it is impossible to describe wherein this consists; the same string too emits different sounds, according as it is struck, touched, or pinched. You can easily distinguish the sound of the horn, the flute, and other musical instruments.

The most wonderful diversity, to say nothing of the variety of articulation in speech, is observable in the human voice, that astonishing masterpiece of the Creator. Reflect but for a moment on the different vowels which the mouth simply pronounces or sings. When the vowel *a* is pronounced or sung, the sound is quite different from that of *e*, *i*, *o*, *u*, or *ai* pronounced or sung, though on the same tone. We must not then look for the reason of this difference in the rapidity or order of the vibrations; no investigation of philosophers has hitherto unfolded this mystery.

You must be perfectly sensible, that in order to utter these different vowels, a different conformation must be given to the cavity of the mouth; and that in man the organization of this part is much better adapted to produce these effects than that of animals. We find, accordingly, that certain birds which learn to imitate the human voice are never capable of distinctly pronouncing the different vowels; the imitation is at best extremely imperfect.

In many organs there is a stop which bears the name of the human voice; it usually, however, contains only the notes which express the vocal sounds *ai* or *ae*. I have no doubt, that with some change it might be possible to produce likewise the other vocal sounds *a*, *e*, *i*, *o*, *u*, *ou;* but even this would not be sufficient to imitate a single word of the human voice; for how can we combine them with the consonants, which are so many modifications of the vowels? We are so conformed, that however common the practice, it is almost impossible to trace and explain the real mechanism.

We distinctly observe three organs employed in expressing the consonants, the lips, the tongue, and the palate; but the nose likewise essentially concurs. On stopping it, we become incapable of pronouncing the letters *m* and *n;* the sound of *b* and *d* only is then to be heard. A striking proof of the marvellous structure of our mouth for the pronunciation of the letters undoubtedly is, that all the skill of man has not hitherto been capable of producing a piece of mechanism that could imitate it. The song has been exactly imitated, but without any articulation of sounds, and without distinction of the different vowels.

The construction of a machine capable of expressing sounds, with all the articulations, would no doubt be a very important discovery. Were it possible to execute such a piece of mechanism, and bring it to such perfection that it could pronounce all words, by means of certain stops, like those of an organ or harpsichord, every one would be surprised, and justly, to hear a machine pronounce whole discourses or sermons together, with the most graceful accompaniments. Preachers and other orators, whose voice is either too weak or disagreeable, might play their sermons or orations on such a machine, as organists do pieces of music. The thing does not seem to me impossible.[*]

16*th June*, 1761.

LETTER XXIII.

A Summary of the principal Phenomena of Electricity.[*]

The subject which I am now going to recommend to your attention almost terrifies me. The variety it presents is immense, and the enumeration of facts serves rather to confound than to inform. The subject I mean is electricity, which for some time past has become an object of such importance in physics that every one is supposed to be acquainted with its effects.

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[*] Pipes have actually been constructed of such forms, by Kratzenstein and Kempelen, as to imitate very accurately the different vowel sounds produced by the human voice. From this first attempt Kempelen proceeded to analyze the mechanism of speech, and he succeeded in constructing a speaking machine, which uttered, not only words, but entire sentences. The four letters D, G, K, T, however, baffled all his ingenuity; and he was obliged to substitute for them the letter P, which was so managed as to bear a considerable resemblance to them, so much so, at least, as to deceive the auditory.—See the *Edinburgh Encyclopædia*, article ACOUSTICS, vol. i. p. 126; and AUTOMATON, vol. iii. p. 153, where a full account of this machine is given.—*Ed.*

The ingenuity of the Swiss mechanicians in constructing artificial birds, dogs, and other animals which emit sounds so nearly resembling those of their prototypes as to deceive many ears, is known to those who have visited the workshops of Geneva, Locle, Chande-fond, and other towns. See on this subject Brewster's Natural Magic, No. L. Harper's Family Library.—*Am. Ed.*

You must undoubtedly have frequently heard it mentioned in conversation; but I know not whether you have ever witnessed any of the experiments. Natural philosophers of modern times prosecute the study of it with ardour, and are almost every day discovering new phenomena, the description of which would employ many hundreds of letters; nay, perhaps I should never have done.

And here it is I am embarrassed. I could not bear to think of letting you remain unacquainted with a branch of natural philosophy so essential; but I would willingly save you the fatigue of wading through a diffuse detail of the phenomena, which after all would not furnish the necessary information. I flatter myself, however, that I have discovered a road which will lead so directly to the object, that you shall attain a knowledge of it much more perfect than that of most natural philosophers, who devote night and day to the investigation of these mysteries of nature.

Without stopping to explain the various appearances and effects of electricity, which would engage me in a long and tedious detail, without extending your knowledge of the causes which produce these effects, I shall pursue quite a different course, and begin with unfolding the true principle of nature on which all these phenomena are founded, however various they may appear, and from which they are all easily deducible.

It is sufficient to remark, in general, that *electricity* is excited by the *friction of a glass tube*. It thereby becomes electrical: and then it alternately attracts and repels light bodies which are applied to it; and on the application of other bodies, *sparks of fire* are mutually extracted, which, increased in strength, kindle spirits of wine and other combustible substances. On touching such a tube with the finger, you feel, besides the spark, a puncture which may in certain circumstances be rendered so acute as to produce a commotion through the whole body.

Instead of a tube of glass, we likewise employ a globe of glass, which is made to turn round an axis like a turning-wheel. During this motion it is rubbed with the hand, or with a cushion applied to it; then the globe becomes electric, and produces the same phenomena as the tube.

Besides glass, resinous bodies, such as Spanish wax, and sulphur, likewise possess the property of becoming electric by friction; but certain species of bodies only have this quality, of which glass, sealing-wax, and sulphur are the principal.

Other bodies undergo friction without producing any such effect; no sign of electricity appears: but on applying them to the first, when rendered electric, they immediately acquire the same property. They become electric, then, by communication, as they touch; and frequently the approximation only of electric bodies renders them such.

All bodies, therefore, are divisible into two classes; in the one are included those that become electric by friction, in the other those which are rendered such by communication, whereas friction produces no manner of effect on them. It is very remarkable that bodies of the first class receive no electricity from communication; when you apply to a tube or globe of glass strongly electrified, other glasses or bodies which friction renders electric, this touch communicates no electricity to them. The distinction of these two classes of bodies is worthy of attention; the one class being disposed to become electrical by friction only, and not by communication—the other, on the contrary, only by communication.*

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* The distinction between these two classes is not so absolute as the author's remark would lead the student to infer. The classes are, with proper precaution, convertible into each other.—*Am. Ed.*

All metals belong to this last class, and the communication extends so far, that on presenting one extremity of a wire to an electric body, the other extremity becomes so likewise, be the wire ever so long; and on applying still another wire to the farther extremity of the first, the electricity is conveyed through the whole extent of that second thread—and thus electricity may be transmitted to the most remote distances.

Water is a substance which receives electricity by communication. Large pools have been electrified to such a degree that the application of the finger has elicited sparks and excited pain.

The prevailing persuasion now is, that lightning and thunder are the effect of the electricity which the clouds acquire, from whatever cause. A thunder-storm exhibits the same phenomena of electricity, on the great scale, which the experiments of natural philosophers do in miniature.

20th June, 1761.

LETTER XXIV.

The true Principle of Nature on which are founded all the Phenomena of Electricity.

The summary I have exhibited of the principal phenomena of electricity has no doubt excited a curiosity to know what occult powers of nature are capable of producing effects so surprising.

The greatest part of natural philosophers acknowledge their ignorance in this respect. They appear to be so dazzled by the endless variety of phenomena which every day present themselves, and by the singularly marvellous circumstances which accompany these phenomena, that they are discouraged from attempting an investigation of the true cause of them. They readily admit the existence of a subtile matter, which is the primary agent in the production of the phenomena, and which they denominate the electric fluid; but they are so embarrassed about determining its nature and properties, that this important branch of physics is rendered only more perplexed by their researches.

There is no room to doubt that we must look for the source of all the phenomena of electricity only in a certain fluid and subtile matter; but we have no need to go to the regions of imagination in quest of it. That subtile matter denominated *ether*, whose reality I have already endeavoured to demonstrate,* is sufficient very naturally to explain all the surprising effects which electricity presents. I hope I shall be able to set this in so clear a light, that you shall be able to account for every electrical phenomenon, however strange an appearance it may assume.

The great requisite is to have a thorough knowledge of the nature of ether. The air which we breathe rises only to a certain height above the surface of the earth; the higher you ascend the more subtile it becomes, and at last it entirely ceases. We must not affirm that beyond the region of the air there is a perfect vacuum which occupies the immense space in which the heavenly bodies revolve. The rays of light, which are diffused in all directions from these heavenly bodies, sufficiently demonstrate that those vast spaces are filled with a subtile matter.

If the rays of light are emanations forcibly projected from luminous bodies, as some philosophers have maintained, it must follow that the whole space of the heavens is filled with these rays—nay, that they move through it with incredible rapidity. You have only to recollect the prodigious velocity with which the rays of the sun are transmitted to us. On this hypothesis, not only would there be no

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* See Letter XV. vol. I.

vacuum, but all space would be filled with a subtile matter, and that in a state of constant and most dreadful agitation.

But I think I have clearly proved that rays of light are no more emanations projected from luminous bodies than sound is from sonorous bodies. It is much more certain that rays of light are nothing else but a tremulous motion or agitation of a subtile matter, just as sound consists of a similar agitation excited in the air. And as sound is produced and transmitted by the air, light is produced and transmitted by that matter, incomparably more subtile, denominated ether, which consequently fills the immense space between the heavenly bodies.

Ether, then, is a medium proper for the transmission of rays of light: and this same quality puts us in a condition to extend our knowledge of its nature and properties. We have only to reflect on the properties of air, which render it adapted to the reception and transmission of sound. The principal cause is its elasticity or spring. You know that air has a power of expanding itself in all directions, and that it does expand the instant that obstacles are removed. The air is never at rest but when its elasticity is everywhere the same; whenever it is greater in one place than another the air immediately expands. We likewise discover by experiment that the more the air is compressed, the more its elasticity increases: hence the force of air-guns, in which the air, being very strongly compressed, is capable of discharging the ball with astonishing velocity. The contrary takes place when the air is rarefied: its elasticity becomes less in proportion as it is more rarefied, or diffused over a larger space.

On the elasticity of the air, then, relative to its density, depends the velocity of sound, which makes a progress of 1142 feet in a second. If the elasticity of the air were increased, its density remaining the same, the velocity of sound would increase; and the same thing would take place if the air were more rare or less dense than it is, its elasticity being the same. In general, the more that any medium, similar to air, is elastic, and at the same time less dense, the more rapidly will the agitations excited in it be transmitted. And as light is transmitted so many thousand times more rapidly than sound, it must clearly follow that the ether, that medium whose agitations constitute light, is many thousand times more elastic than air, and, at the same time, many thousand times more rare or more subtile, both of these qualities contributing to accelerate the propagation of light.

Such are the reasons which lead us to conclude that ether is many thousand times more elastic and more subtile than air; its nature being in other respects similar to that of air, in as much as it is likewise a fluid matter, and susceptible of compression and of rarefaction. It is this quality which will conduct us to the explanation of all the phenomena of electricity.

23*d June*, 1761.

LETTER XXV.

Continuation. Different Nature of Bodies relatively to Electricity.

Ether being a subtile matter and similar to air, but many thousand times more rare and more elastic, it cannot be at rest, unless its elasticity, or the force with which it tends to expand, be the same everywhere.

As soon as the ether in one place shall be more elastic than in another, which is the case when it is more compressed there, it will expand itself into the parts adjacent, compressing what it finds there till the whole is reduced to the same degree of elasticity.

It is then in equilibrio, the equilibrium being nothing else but the state of rest, when the powers which have a tendency to disturb it counterbalance each other.

When, therefore, the ether is not in equilibrio the same thing must take place as in air, when its equilibrium is disturbed; it must expand itself from the place where its elasticity is greater towards that where it is less; but, considering its greater elasticity and subtilty, this motion must be much more rapid than that of air. The want of equilibrium in the air produces wind, or the motion of that fluid from one place to another. There must therefore be produced a species of wind, but incomparably more subtile than that of air, when the equilibrium of the ether is disturbed, by which this last fluid will pass from places where it was more compressed and more elastic to those where it was less so.

This being laid down, I with confidence affirm that all the phenomena of electricity are a natural consequence of want of equilibrium in the ether, so that wherever the equilibrium of the ether is disturbed the phenomena of electricity must take place; consequently, electricity is nothing else but a derangement of the equilibrium of the ether.

In order to unfold all the effects of electricity, we must attend to the manner in which ether is blended and enveloped with all the bodies which surround us. Ether, in these lower regions, is to be found only in the small interstices which the particles of the air and of other bodies leave unoccupied. Nothing can be more natural than that the ether, from its extreme subtility and elasticity, should insinuate itself into the smallest pores of bodies which are impervious to air, and even into those of the air itself. You will recollect that all bodies, however solid they may appear, are full of pores; and many experiments incontestably demonstrate that these interstices occupy much more space than the solid parts; finally, the less ponderous a body is, the more it must be filled with these pores, which contain ether only. It is clear, therefore, that though the ether be thus diffused through the smallest pores of bodies, it must however be found in very great abundance in the vicinity of the earth.

You will easily comprehend that the difference of these pores must be very great, both as to magnitude and figure, according to the different nature of the bodies, as their diversity probably depends on the diversity of their pores. There must be, therefore, undoubtedly, pores more close, and which have less communication with others; so that the ether which they contain is likewise more confined, and cannot disengage itself but with great difficulty, though its elasticity may be much greater than that of the ether which is lodged in the adjoining pores. There must be, on the contrary, pores abundantly open, and of easy communication with the adjacent pores; in this case it is evident that the ether lodged in them can with less difficulty disengage itself than in the preceding; and if it is more or less elastic in these than in the others, it will soon recover its equilibrium.

In order to distinguish these two classes of pores, I shall denominate the first *close*, and the others *open*. Most bodies must contain pores of an intermediate species, which it will be sufficient to distinguish by the terms *more* or *less close*, and *more* or *less open*.

This being laid down, I remark, first, that if all bodies had pores perfectly close, it would be impossible to change the elasticity of the air contained in them; and even though the ether in some of these pores should have acquired, from whatever cause, a higher degree of elasticity than the others, it would always remain in that state, and never recover its equilibrium, from a total want of communication. In this case no change could take place in bodies; all would remain in the same state as if the ether

were in equilibrio, and no phenomenon of electricity could be produced.

This would likewise be the case if the pores of all bodies were perfectly open; for then, though the ether might be more or less elastic in some pores than in others, the equilibrium would be instantly restored, from the entire freedom of communication —and that so rapidly that we should not be in a condition to remark the slightest change. For the same reason it would be impossible to disturb the equilibrium of the ether contained in such pores; as often as the equilibrium might be disturbed, it would be as instantaneously restored, and no sign of electricity would be discoverable.

The pores of all bodies being neither perfectly close nor perfectly open, it will always be possible to disturb the equilibrium of the ether which they contain: and when this happens, from whatever cause, the equilibrium cannot fail to re-establish itself; but this re-establishment will require some time, and this produces certain phenomena; and you will presently see, much to your satisfaction, that they are precisely the same which electrical experiments have discovered. It will then appear that the principles on which I am going to establish the theory of electricity are extremely simple, and at the same time absolutely incontrovertible.

27th June, 1761.

LETTER XXVI.

On the same Subject.

I hope I have now surmounted the most formidable difficulties which present themselves in the theory of electricity. You have only to preserve the idea of ether which I have been explaining; and which is, that extremely subtile and elastic matter diffused, not only through all the void spaces of the universe, but through the minutest pores of all bodies in which it is sometimes more and sometimes less engaged, according as they are more or less close. This consideration conducts us to two principal species of bodies, of which the one has pores more close, and the other pores more open.

Should it happen, therefore, that the ether contained in the pores of bodies has not throughout the same degree of elasticity, and that it is more or less compressed in some than in others, it will make an effort to recover its equilibrium; and it is precisely from this that the phenomena of electricity take their rise, which, of consequence, will be varied in proportion as the pores in which the ether is lodged are various, and grant it a communication more or less free with the others.

This difference in the pores of bodies perfectly corresponds to that which the first phenomena of electricity have made us to remark in them, by which some easily become electrical by communication, or the proximity of an electrical body, whereas others scarcely undergo any change. Hence you will immediately infer that bodies which receive electricity so easily by communication alone are those whose pores are open; and that the others, which are almost insensible to electricity, must have theirs close, either entirely or to a very great degree.

It is, then, by the phenomena of electricity themselves that we are enabled to conclude what are the bodies whose pores are close or open. Respecting which permit me to suggest the following elucidations.

First, the air which we breathe has its pores almost entirely close; so that the ether which it contains cannot disengage itself but with difficulty, and must find equal difficulty in attempting to penetrate into it. Thus, though the ether diffused through the air is not in equilibrio with that which is contained in other bodies where it is more or less compressed, the re-establishment of its equilibrium is not to be produced without extreme difficulty; this is to be understood of dry air, humidity being of a different nature, as I shall presently remark.

Further, we must rank in this class of bodies with close pores, *glass*, *pitch*, *resinous bodies*, *sealing-wax*, *sulphur*, and particularly *silk*. These substances have their pores so very close that it is with extreme difficulty the ether can either escape from or penetrate into them.

The other class, that of bodies whose pores are open, contains, first, water and other liquors, whose nature is totally different from that of air. For this reason, when air becomes humid it totally changes its nature with respect to electricity, and the ether can enter or escape without almost any difficulty. To this class of bodies with open pores likewise must be referred those of animals, and all metals.

Other bodies, such as wood, several sorts of stones and earths, occupy an intermediate state between the two principal species which I have just mentioned; and the ether is capable of entering or escaping with more or less facility, according to the nature of each species.

After these elucidations on the different nature of bodies with respect to the ether which they contain, you will see with much satisfaction how all the phenomena of electricity, which have been considered as so many prodigies, flow very naturally from them.

All depends, then, on the state of the ether diffused or dispersed through the pores of all bodies, in as far as it has not throughout the same degree of elasticity, or as it is more or less compressed in some than in others: for the ether not being then in equilibrio will make an effort to recover it. It will endeavour to disengage itself as far as the openness of the pores will permit from places where it is too much compressed, to expand itself and enter into pores where there is less compression, till it is throughout reduced to the same degree of compression and elasticity, and is, of consequence, in equilibrio.

Let it be remarked, that when the ether passes from a body where it was too much compressed into another where it is less so, it meets with great obstacles in the air which separates the two bodies on account of the pores of this fluid, which are almost entirely close. It however passes through the air as a liquid and extremely subtile matter, provided its force is not inferior, or the interval between the bodies too great. Now, this passage of the ether being very much impeded, and almost entirely prevented by the pores of the air, the same thing will happen to it as to air forced with velocity through small apertures—a hissing sound is heard—which proves that this fluid is then put into an agitation which produces such a sound.

It is, therefore, extremely natural that the ether, forced to penetrate through the pores of the air, should likewise receive a species of agitation. You will please to recollect, that as agitation of the air produces sound, a similar agitation of ether produces light. As often, then, as ether escapes from one body to enter into another, its passage through the air must be accompanied with light; which appears sometimes under the form of a spark, sometimes under that of a flash of lightning, according as its quantity is more or less considerable.

Here, then, is the most remarkable circumstance which accompanies most electrical phenomena, explained to a demonstration, on the principles I have laid down.* I shall now enter into a more particular detail, which will furnish me with a very agreeable subject for some following Letters.

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* To those conversant with the various phenomena of electricity, the author's theory of close and open pores, and of the different degrees of compression of his supposed ether, will be deemed to fall very far short of a demonstration.—*Am. Ed.*

30*th June*, 1761.

LETTER XXVII.

Of Positive and Negative Electricity. Explanation of the Phenomenon of Attraction.

You will easily comprehend, from what I have above advanced, that a body must become electrical whenever the ether contained in its pores becomes more or less elastic than that which is lodged in adjacent bodies. This takes place when a greater quantity of ether is introduced into the pores of such body, or when part of the ether which it contained is forced out. In the former case, the ether becomes more compressed, and consequently more elastic; in the other, it becomes rarer, and loses its elasticity. In both cases it is no longer in equilibrio with that which is external; and the efforts which it makes to recover its equilibrium produce all the phenomena of electricity.

You see, then, that a body may become electric in two different ways, according as the ether contained in its pores becomes more or less elastic than that which is external; hence result two species of electricity: the one, by which the ether is rendered more elastic, or more compressed, is denominated *increased* or *positive electricity*; the other, in which the ether is less elastic, or more rarefied, is denominated *diminished* or *negative electricity*. The phenomena of both are nearly the same; a slight difference only is observable, which I shall mention.

Bodies are not naturally electrical—as the elasticity of the ether has a tendency to maintain it in equilibrio, it must always require a violent operation to disturb this equilibrium, and to render bodies electrical; and such operations must act on bodies with close pores, that the equilibrium, once deranged, may not be instantly restored. We accordingly find that glass, amber, sealing-wax, or sulphur are the bodies employed to excite electricity.

The easiest operation and for some time past, the most universally known, is to rub a stick of sealing-wax with a piece of woollen cloth, in order to communicate to that wax the power of attracting small slips of paper and of other light bodies. Amber, by means of friction, produces the same phenomena; and as the ancients gave to this body the name of *electrum*, the power excited by friction obtained, and preserves, the name of *electricity*—natural philosophers of the remotest ages having remarked that this substance acquired by friction the faculty of attracting light bodies.

This effect undoubtedly arises from the derangement of the equilibrium of the ether by means of friction. I must begin, therefore, with explaining this well-known experiment. Amber and sealing-wax have their pores abundantly close, and those of wool are abundantly open; during the friction, the pores of both the one and the other compress themselves, and the ether which is contained in them is reduced to a higher degree of elasticity. According as the pores of the wool are susceptible of a compression greater or less than those of amber or sealing-wax, it must happen that a portion of ether shall pass from the wool into the amber, or, reciprocally, from the amber into the wool. In the former case, the amber becomes *positively* electric, and in the other *negatively*—and its pores being close, it will remain in this state for some time; whereas the wool, though it has undergone a similar change, will presently recover its natural state.

From the experiments which electric sealing-wax furnishes, we conclude that its electricity is *negative*, and that a part of its ether has passed during the

friction into the wool. Hence you perceive how a stick of sealing-wax is, by friction on woollen cloth, deprived of part of the ether which it contained, and must thereby become electric. Let us now see what effects must result from this, and how far they correspond with observation and experience.

Fig. 39.

Let A B, *Fig.* 39, be a stick of sealing-wax, from which, by friction, part of the ether contained in its pores has been forced out; that which remains, being less compressed, will therefore have less force to expand itself, or, in other words, will have less elasticity than that contained in other bodies in the circumambient air: but as the pores of air are still closer than those of sealing-wax, this prevents the ether contained in the air from passing into the sealing-wax, to restore the equilibrium: at least this will not take place till after a considerable interval of time.

Let a small and very light body C, whose pores are open, be now presented to the stick of sealing-wax, the ether contained in them, finding a free passage, because it has more force to expand itself than is opposed to it by the ether shut up in the stick at *c*, will suddenly escape, will force a passage for itself through the air, provided the distance is not too great, and will enter into the sealing-wax. This passage, however, will not be effected without very considerable difficulty, as the pores of the sealing-wax have only a very small aperture, and consequently it will not be accompanied with a vehemence capable of putting the ether in a motion of agitation, to excite a sensible light. A faint glimmering only will be perceptible in the dark, if the electricity is sufficiently strong.

But another phenomenon will be observable which is no less surprising—the small body C will spring towards the sealing-wax as if attracted by it. To explain the cause of this, you have only to consider that the small body C, in its natural state, is equally pressed on all sides by the air which surrounds it; but as in its present state the ether makes its escape and passes through the air in the direction C *c*, it is evident that this last fluid will not press so violently on the small body on this side as on any other, and that the pressure communicated to it towards *c* will be more powerful than in any other direction, impelling it towards the sealing-wax as if attracted by it.

Thus are explained, in a manner perfectly intelligible, the attractions observable in the phenomena of electricity. In this experiment, the electricity is too feeble to produce more surprising effects. I shall have the honour of presenting you with a more ample detail in the following Letters.

4th July, 1761.

LETTER XXVIII.

On the same Subject.

Such were the faint beginnings of the electrical phenomena; it was not till lately that they were carried much farther. At first a tube of glass was employed, similar to that of which barometers are made, but of a larger diameter, which was rubbed with the naked hand, or with a piece of woollen cloth, and electrical phenomena more striking were observed.

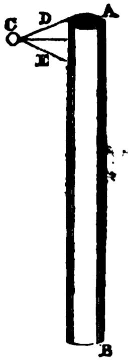
You will readily comprehend, that on rubbing a tube of glass, part of the ether must pass, in virtue

of the compression of the pores of the glass, and of the rubbing body, either from the hand into the glass, or from the glass into the hand, according as the pores of the one or of the other are more susceptible of compression in the friction. The ether, after this operation, easily recovers its equilibrium in the hand, because its pores are open; but those of the glass being abundantly close, this fluid will preserve its state in it, whether the glass is surcharged or exhausted, and consequently will be electric, and will produce phenomena similar to those of sealing-wax, but undoubtedly much stronger, as its electricity is carried to a higher degree, as well from the greater diameter of the tube as from the very nature of glass.

Experiments give us reason to conclude that the tube of glass becomes, by these means, surcharged with ether, whereas sealing-wax is exhausted of it; the phenomena however are nearly the same.

It must be observed that the glass tube retains its electricity as long as it is surrounded only with air, because the pores of the glass and those of the air are too close to allow a communication sufficiently free to the ether, and to exhaust the glass of what it has more than in its natural state; superfluity of ether always increasing elasticity. But the air must be very dry, for only when in that state are its pores sufficiently close; when it is humid or loaded with vapours, experiments do not succeed, whatever degree of friction you bestow on the glass. The reason is obvious; for water, which renders the air humid, having its pores very open, receives every instant the superfluous ether which was in the glass, and which of course remains in its natural state. Experiments succeed, then, in only very dry air: let us now see what phenomena a glass tube will in that case produce, after having undergone considerable friction.

Fig. 40.



It is clear that on presenting to it a small light body C, *Fig.* 40, with open pores, such as gold leaf, the ether in the tube, more elastic at the nearest parts, D, E, will not make ineffectual efforts to discharge itself and pass into the pores of the body C. It will force a path through the air, provided the distance be not too great; and you will even see a light between the tube and the body, occasioned by the agitation excited in the ether, which passes with difficulty from the tube into the body. When, instead of the body C, the finger is applied to it, you feel a pricking, occasioned by the rapid entrance of the ether; and if you expose your face to it at some distance, you feel a certain agitation in the air, excited by the transition of the ether. These circumstances are likewise accompanied sometimes with a slight crackling, produced undoubtedly by the agitation of the air, which the ether traverses with such rapidity.

I must entreat you to keep in mind, that an agitation in the air always produces a sound, and that the motion of ether produces light; and then the explanation of these phenomena will become abundantly easy.

Let the small light body C be replaced in the vicinity of our electric tube; as long as the ether is escaping from the tube, to enter into the pores of the body C, the air will be in part expelled from it, and consequently will not press so strongly on the body on that side as in every other direction; it will happen, then, as in the preceding case, that the body C will be impelled towards the tube, and being light, will come close up to it. We see, then, that this apparent attraction equally takes place, whether the ether in the tube be too much or too little elastic,

or whether the elasticity of the tube be positive or negative. In both cases, the passage of the ether stops the air, and by its pressure hinders it from acting.

But while the small body C is approaching the tube, the passage of the ether becomes stronger, and the body C will soon be as much surcharged with ether as the tube itself. Then the action of the ether, which arises from its elasticity only, entirely ceases, and the body C will sustain on all sides an equal pressure. The attraction will cease, and the body C will remove from the tube, as nothing detains it, and its own gravity puts it in motion. Now, as soon as it removes, its pores being open, its superfluous ether presently escapes in the air, and it returns to its natural state. The body will then act as at the beginning, and you will see it again approach the tube, so that it will appear alternately attracted and repelled by it; and this play will go on till the tube has lost its electricity. For as, on every attraction, it discharges some portion of its superfluous ether, besides the insensible escape of part of it in the air, the tube will soon be re-established in its natural state, and in its equilibrium; and this so much the more speedily as the tube is small, and the body C light; then all the phenomena of electricity will cease.

7th July, 1761.

LETTER XXIX.

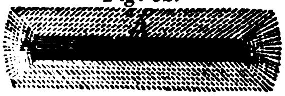
On the Electric Atmosphere.

I HAD almost forgotten to bring forward a most essential circumstance, which accompanies all electric bodies, whether *positively* or *negatively* such, and which supplies some very striking elucidations for explaining the phenomena of electricity.

Though it be indubitably true that the pores of air are very close, and scarcely permit any communication between the ether that they contain and what is in the vicinity, it undergoes, however, some change when near to an electric body.

Let us first consider an electric body *negatively* so, as a stick of *sealing-wax* A B, *Fig.* 92, which has been deprived by friction of part of the ether contained in its pores, so that what it now contains has less elasticity than that of other bodies, and consequently than that of the air which surrounds the wax. It must necessarily happen, that the ether contained in the particles of the air which immediately touch the wax, as at *m*, having greater elasticity, should discharge itself, in however small a degree, into the pores of the wax, and will consequently lose somewhat of its elasticity. In like manner, the particles of air more remote, suppose at *n*, will likewise suffer a portion of their ether to escape into the nearer at *m*, and so on to a certain distance beyond which the air will no longer undergo any change. In this manner, the air round the stick of wax to a certain distance will be deprived of part of its ether, and become itself electric.

Fig. 92.



This portion of the air, which thus partakes of the electricity of the stick of wax, is denominated the *electric atmosphere;* and you will see from the proofs which I have just adduced, that every electric body must be surrounded with an atmosphere. For if the body is *positively* electric, so as to contain a superfluity of ether, it will be more compressed in such a body, and consequently more elastic, as is the case with a tube of glass when rubbed; this ether, more elastic, then discharges itself, in a small degree, into the particles of air which immediately touch it,

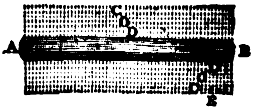
and thence into particles more remote, to a certain distance; this will form another electric atmosphere round the tube, in which the ether will be more compressed, and consequently more elastic than elsewhere.

It is evident that this atmosphere which surrounds such bodies must gradually diminish the electricity of them, as in the first case there passes almost continually a small portion of ether from the surrounding air into the electric body, and which, in the other case, issues from the electric body and passes into the air. This is likewise the reason why electric bodies at length lose their electricity; and this so much the sooner, as the pores of the air are more open. In a humid air, whose pores are very open, all electricity is almost instantly extinguished; but in very dry air it continues a considerable time.

This electric atmosphere becomes abundantly sensible on applying your face to an electrified body; you have a feeling similar to the application of a spider's web, occasioned by the gentle transition of the ether from the face into the electric body, or reciprocally, from this last into the face, according as it is negative or positive, to use the common expression.

The electric atmosphere serves likewise more clearly to explain that alternate attraction and repulsion of light bodies placed near to electric bodies which I mentioned in the preceding Letter; in which you must have remarked, that the explanation of repulsion there given is incomplete; but the electric atmosphere will supply the defect.

Fig. 93.



Let A B, *Fig.* 93, represent an electric tube of glass surcharged with ether, and let C be a small light body, with pores sufficiently open, in its natural state. Let the atmosphere extend as far as the distance D E. Now, as the vicinity of C contains already an ether more elastic, this will discharge itself into the pores of the body C; there will immediately issue from the tube a new ether, which will pass from D into C, and it is the atmosphere chiefly which facilitates this passage. For if the ether contained in the air had no communication with that in the tube, the corpuscle C would not feel the vicinity of the tube; but while the ether is passing from D to C, the pressure of the air between C and D will be diminished, and the corpuscle C will no longer be pressed equally in all directions; it will therefore be impelled towards D, as if attracted by it. Now, in proportion as it approaches, it will be likewise more and more surcharged with ether, and will become electric as the tube itself, and consequently the electricity of the tube will no longer act upon it.

But as the corpuscle, being now arrived at D, contains too much ether, and more than the air at E, it will have a tendency to escape, in order to make its way to E. The atmosphere in which the compression of the ether continues to diminish from D to E will facilitate this passage, and the superfluous ether will in effect flow from the corpuscle towards E. By this passage, the pressure of the air on the corpuscle will be smaller on that side than everywhere else, and consequently the corpuscle will be carried towards D, as if the tube repelled it. But as soon as it arrives at E, it discharges the superfluous ether, and recovers its natural state; it will then be again attracted towards the tube, and having reached it, will be again repelled, for the reason which I have just been explaining.

It is the electric atmosphere then chiefly which produces these singular phenomena, when we see electrified bodies alternately attract and repel small light bodies, such as little slips of paper, or particles

of metal, with which this experiment best succeeds, as the substances have very open pores.

You will see, moreover, that what I have just now said respecting *positive* electricity must equally take place in *negative*. The transition of the ether is only reversed, by which the natural pressure of the air must always be diminished.

11*th July*, 1761.

LETTER XXX.

Communication of Electricity to a Bar of Iron, by means of a Globe of Glass.

After the experiments made with glass tubes, we have proceeded to carry electricity to a higher degree of strength. Instead of a tube, a globe or round ball of glass has been employed, which is made to turn with great velocity round an axis, and on applying the hand to it, or a cushion of some matter with open pores, a friction is produced which renders the globe completely electric.

Fig. 94.

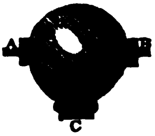


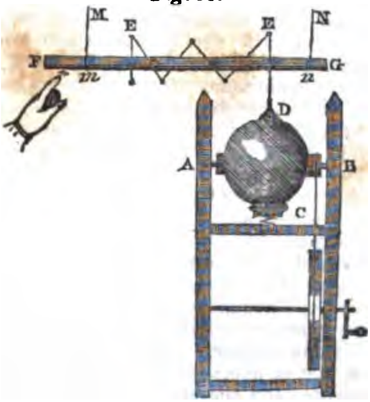
Fig. 94 represents this globe, which may be made to move round an axis A B, by a mechanism similar to that employed by turners. C is the cushion strongly applied to the globe, on which it rubs as it turns round. The pores of the cushion being, in this friction, compressed more than those of the glass, the ether contained in it is expelled, and forced to insinuate itself into the pores of the glass, where they continue to accumulate, because the open pores of the cushion are continually supplying it with more ether, which it is extracting, at least in part, from surrounding bodies; and thus the globe may be surcharged with ether to a much higher degree than glass tubes.

The effects of electricity are accordingly rendered much more considerable, but of the same nature with those which I have described, alternately attracting and repelling light bodies; and the sparks which we see on touching the electric globe are much more lively.

But naturalists have not rested satisfied with such experiments, but have employed the electrical globe in the discovery of phenomena much more surprising.

Having constructed the machine for turning the globe round its axis A B, a bar of iron F G, *Fig.* 95,

Fig. 95.



is suspended above, or on one side of the globe, and towards the globe is directed a chain of iron or other metal E D, terminating at D, in metallic filaments, which touch the globe. It is sufficient that this chain be attached to the bar of iron in any manner whatever, or but touch it. When the globe is turned round, and in turning made to rub on the cushion at

C, in order that the glass may become surcharged with ether, which will consequently be more elastic, it will easily pass from thence into the filaments D, for, being of metal, their pores are very open; and from thence, again, it will discharge itself by the chain D E, into the bar of iron F G. Thus, by means of the globe, the ether extracted from the cushion C will successively accumulate in the bar of iron F G, which likewise, of consequence, becomes electric; and its electricity increases in proportion as you continue to turn the globe.

If this bar had a further communication with other bodies whose pores too are open, it would soon discharge into them its superfluous ether, and thereby lose its electricity; the ether extracted from the cushion would be dispersed over all the bodies which had an intercommunication, and its greatest compression would not be more perceptible. To prevent this, which would prove fatal to all the phenomena of electricity, the bar must of necessity be supported or suspended by props of a substance whose pores are very close; such as glass, pitch, sulphur, sealing-wax, and silk. It would be proper, then, to support the bar on props of glass or pitch, or to suspend it by cords of silk. The bar is thus secured against the transmission of its accumulated ether, as it is surrounded on all sides only by bodies with close pores, which permit hardly any admission to the ether in the bar. The bar is then said to be *insulated*, that is, deprived of all contact which could communicate, and thereby diminish, its electricity. You must be sensible, however, that it is not possible absolutely to prevent all waste; for this reason, the electricity of such a bar must continually diminish, if it were not kept up by the motion of the globe.

In this manner electricity may be communicated to a bar of iron, which never could be done by the most violent and persevering friction, because of the openness of its pores. And, for the same reason, such a bar rendered electric by communication produces phenomena much more surprising. On presenting to it a finger, or any other part of the body, you see a very brilliant spark dart from it, which, entering into the body, excites a pungent and sometimes painful sensation. I recollect having once presented to it my head, covered with my peruke and hat, and the stroke penetrated it so acutely that I felt the pain next day.[*]

These sparks, which escape from every part of the bar on presenting to it a body with open pores, set on fire at once spirit of wine, and kill small birds whose heads are exposed to them. On plunging the end of the chain D E into a basin filled with water, and supported by bodies with close pores, such as glass, pitch, silk, the whole water becomes electric; and some authors assure us that they have seen considerable lakes electrified in this manner, so that on applying the hand you might have seen even very pungent sparks emitted from the water. But it appears to me that the globe must be turned a very long time indeed, to convey such a portion of ether into a mass of water so enormous; it would be likewise necessary that the bed of the lake, and every thing in contact with it, should have their pores close.[†]

The more open, then, the pores of a body are, the more disposed it is to receive a higher degree of electricity, and to produce prodigious effects. You must admit that all this is perfectly conformable to the principles which I at first established.

14*th July*, 1761.

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[*] In the early period of the science, the results of electric action were so new and surprising that the imaginations of many persons were highly wrought upon by them. Muschenbroeck asserted, it is said, that he would not take a second shock for the kingdom of France.—*Am. Ed.*

[†] Such an effect as the author alludes to is not in the least degree probable.—*Am. Ed.*

LETTER XXXI.

Electrization of Men and Animals.

As electricity may be communicated from glass to a bar of iron, by means of a chain which forms that communication, it may likewise be conveyed into the human body; for the bodies of animals have this property in common with metals and water, that their pores are very open; but the man who is to be the subject of the experiment must not be in contact with other bodies whose pores are likewise open.

For this purpose, the man is placed on a large lump of pitch, or seated on a chair supported by glass columns, or a chair suspended by cords of silk, as all these substances have pores sufficiently close to prevent the escape of the ether with which the body of the man becomes surcharged by electricity.

This precaution is absolutely necessary, for were the man placed on the ground, the pores of which are abundantly open, as soon as the ether was conveyed into his body to a higher degree of compression, it would immediately discharge itself into the earth; and we must be in a condition to surcharge it entirely with ether before the man could become electric. Now you must be sensible that the cushion by which the globe of glass is rubbed could not possibly supply such a prodigious quantity of ether, and that were you to extract it even out of the earth itself, you could gain no ground, for you would just take away as much on the one hand as you gave on the other.

Having then placed the man whom you mean to electrify in the manner which I have indicated, you have only to make him touch with his hand the globe of glass while it turns, and the ether accumulated in the globe will easily pass into the open pores of the hand, and diffuse itself over the whole body, from whence it cannot so easily escape, as the air and all the bodies with which he is surrounded have their pores close. Instead of touching the globe with his hand, it will be sufficient for him to touch the chain, or even the bar, which I described in the preceding Letter; but in this case, not only the man himself must be surcharged with ether, but likewise the chain with the bar of iron; and as this requires a greater quantity of ether, it would be necessary to labour longer in turning the globe, in order to supply a sufficient quantity.*

In this manner the man becomes entirely electric, or, in other words, his whole body will be surcharged with ether; and this fluid will consequently be found there in the highest degree of compression and electricity, and will have a violent tendency to escape.

You must be abundantly sensible that a state so violent cannot be indifferent to the man. The body is in its minutest parts wholly penetrated with ether, and the smallest fibres as well as the nerves are so filled with it, that this ether, without doubt, pervades the principal springs of animal and vital motion. It is accordingly observed, that the pulse of a man electrified beats faster—he is thrown into a sweat—and the motion of the more subtile fluids with which the body is filled becomes more rapid. A certain change is likewise felt over the whole body, which it is impossible to describe; and there is every reason to believe, that this state has a powerful influence on the health, though sufficient experiments have not yet been made to ascertain in what cases this influence is salutary, or otherwise. It may sometimes be

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* This last mode, however, of performing the experiment, would be much the better of the two.—*Am. Ed.*

highly beneficial to have the blood and humours raised to a more lively circulation; certain obstructions, which threaten dangerous consequences, might thereby be prevented; but on other occasions an agitation too violent might prove injurious to health. The subject certainly well deserves the attention of medical gentlemen. We have heard of many surprising cures performed by electricity, but we are not yet enabled sufficiently to distinguish the occasions on which we may promise ourselves success.

To return to our electrified man; it is very remarkable, that in the dark we see him surrounded with a light similar to that which painters throw round the heads of saints. The reason is abundantly obvious; as there is always escaping from the body of that man some part of the ether with which he is surcharged, this fluid meets with much resistance from the close pores of the air; it is thereby put into a certain agitation, which is the origin of light, as I have had the honour to demonstrate.

Phenomena of a very surprising nature are remarked in this state of a man electrified. On touching him, you not only see very brilliant sparks issue from the part which you touch, but the man feels besides a very pungent pain. Further, if the person who touches him be in his natural state, or not electrified, both sensibly feel this pain, which might have fatal consequences, especially if he were touched in the head, or any other part of the body of acute sensibility. You will readily comprehend, then, how little indifferent it is to us, that a part of the ether contained in our body escape from it, or that new ether is introduced, especially as this is done with such amazing rapidity.

Moreover, the light with which we see the man surrounded in the dark is an admirable confirmation of my remarks respecting the electric atmosphere which is diffused round all bodies; and you will no longer find any difficulty in the greater number of electrical phenomena, however inexplicable they may at first appear.

18th July, 1761.

LETTER XXXII.

Distinctive Character of the two Species of Electricity.

You will please to recollect, that not only glass becomes electric by friction, but that other substances, such as sealing-wax and sulphur, have the same property, in as much as their pores are likewise close; so that whether you introduce into them an extraordinary quantity of ether, or extract a part of it, they continue for some time in that state; nor is the equilibrium so soon restored.

Accordingly, instead of a globe of glass, globes of sealing-wax and sulphur are employed, which are likewise made to revolve round an axis, rubbing at the same time against a cushion, in the same manner which I described respecting a globe of glass. Such globes are thus rendered equally electric; and on applying to them a bar of iron, which touches them only by slender filaments or fringes of metal, incapable of injuring the globe, electricity is immediately communicated to that bar, from which you may afterward transmit it to other bodies at pleasure.

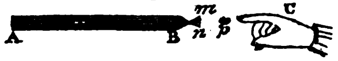
Here, however, a very remarkable difference is observable. A globe of glass rendered electric in this manner becomes surcharged with ether; and the bar of iron, or other bodies brought into communication with it, acquire an electricity of the same nature. This electricity is denominated *positive* or *augmented* electricity. But when a globe of sealing-wax or sulphur is treated in the same manner, an electricity directly opposite is the result, which is denominated *negative* or *diminished* electricity, as it is perceived that by friction these globes are deprived of part of the ether contained in their pores.

You will be surprised to see that the same friction is capable of producing effects altogether opposite; but this depends on the nature of the bodies which undergo the friction, whether by communicating or receiving it, and of the rigidity of their particles which contain the pores. In order to explain the possibility of this difference, it is evident, at first sight, that when two bodies are rubbed violently against each other, the pores of the one must in most cases undergo a greater compression than those of the other, and that then the ether contained in the pores is extruded, and forced to insinuate itself into those of the bodies which are less compressed.

It follows, then, that in this friction of glass against a cushion, the pores of the cushion undergo a greater compression than those of the glass, and consequently the ether of the cushion must pass into the glass, and produce in it a positive or *increased* electricity, as I have already shown. But on substituting a globe of sealing-wax or of sulphur in place of the glass, these substances being susceptible of a greater degree of compression in their pores than the substance of the cushion with which the friction is performed, a part of the ether contained in these globes will be forced out, and constrained to pass into the cushion; the globe of sealing-wax or sulphur will thereby be deprived of part of its ether, and of course receive a negative or *diminished* electricity.

The electricity which a bar of iron, or of any other metal, receives from communication with a globe of sealing-wax or sulphur, is of the same nature; as is also that which is communicated to a man placed on a lump of pitch, or suspended by cords of silk. When such a man, or any other body with open pores electrified in the same manner, is touched, nearly the same phenomena are observable as in the case of positive electricity. The touch is here likewise accompanied with a spark, and a puncture on both sides. The reason is obvious; for the ether which in this case escapes from bodies in their natural state, to enter into electrified bodies, being under constraint, must be under an agitation which produces light. A sensible difference is, however, to be remarked in the figure of the spark, according as the electricity is positive or negative. See that of positive electricity, *Fig.* 96.

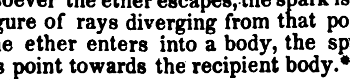
Fig. 96.



If the bar A B possesses positive electricity, and the finger C is presented to it, the light which issues out of the bar appears under the form of rays diverging from the bar towards the finger *m n*, and the luminous point is seen next the finger.

But if the bar A B, *Fig.* 97, is negatively electric,

Fig. 97.



and the finger C is presented to it, the luminous rays *m n* diverge from the finger, and you see the luminous point *p* next the bar.

This is the principal character by which positive is distinguished from negative electricity. From whencesoever the ether escapes, the spark is emitted in the figure of rays diverging from that point; but when the ether enters into a body, the spark is a luminous point towards the recipient body.*

21*st July*, 1761.

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* Professor Hildebrand has lately found that the size and luminousness of the spark depend upon the nature as well as upon the form of the metal from which the sparks are taken. The pieces of metal which he used were of a conical form. They had all the same shape and dimen-

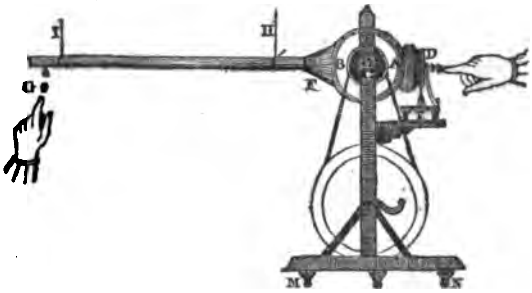
LETTER XXXIII.

How the same Globe of Glass may furnish at once the two Species of Electricity.

You will be enabled to see still more clearly the difference between positive and negative electricity, after I have explained how it is possible to produce by one and the same globe of glass both the species; and this will serve at the same time further to elucidate these wonderful phenomena of nature.

Let A B, *Fig.* 98, be the globe of glass turning

Fig. 98.



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sions, and were fixed in the same manner, at the end of an insulated conductor. The sparks differed very much in extent, as shown in the following list; those at the top of the list giving the greatest sparks, and those at the bottom the least.

Regulus of Antimony.
Gold.
Silver.
Brass.
Sulphuret of Copper.
Tin.
Zinc.
Iron. Lead.
Steel.
Tempered Steel.

A cone with an angle of 52° gave a much more luminous spark than one with an angle of 36°. The parabolic rounding of the summit, or slight inequalities of surface, were found to be particularly favourable to the production of a strong light.—*Ed.*

round its axis C, and rubbed against the cushion D, in an opposite direction to which the globe is touched by the metallic filaments F attached to the bar of iron F G, which is suspended by cords of silk H and I, that it may nowhere touch bodies with open pores.

This being laid down, you know that by friction against the cushion D, the ether passes from the cushion into the glass, from which it becomes more compressed, and consequently more elastic: it will pass, therefore, from thence, by the metallic filaments F, into the bar of iron F G; for though the pores of glass are abundantly close, as the ether in the globe is continually accumulating by the friction, it soon becomes so overcharged that it escapes by the metallic filaments, and discharges itself into the bar, by which this last becomes equally electric.

Hence you perceive that all this superfluity of ether is supplied by the cushion, which would speedily be exhausted unless it had a free communication with the frame which supports the machine, and thereby with the whole earth, which is every instant supplying the cushion with new ether; so that as long as the friction continues, there is a quantity sufficient further to compress that which is in the globe and in the bar. But if the whole machinery is made to rest on pillars of glass, as M and N, or if it is suspended by cords of silk, that the cushion may have no communication with bodies whose pores are open, which might supply the deficiency of ether, it would soon be exhausted, and the electricity could not be conveyed into the globe and the bar beyond a certain degree, which will be scarcely perceptible unless the cushion be of a prodigious size. To supply this defect, the cushion D is put in communication with a large mass of metal E, the ether of which is sufficient to supply the globe and the bar, and to carry it to such a high degree of compression.

You will thus procure to the globe and to the bar a positive electricity, as has been already explained. But in proportion as they become surcharged with ether, the cushion and the metallic mass E will lose the same quantity, and thereby acquire a negative electricity: so that we have here at once the two species of electricity; the positive in the bar, and the negative in the metallic mass. Each produces its effect conformably to its nature. On presenting a finger to the bar, a spark with divergent rays will issue from the bar, and the luminous point will be seen towards the finger; but if you present the finger to the metallic mass, the spark with divergent rays will issue from the finger, and you will see the luminous point towards the mass.

Let us suppose two men placed on lumps of pitch, to cut off all communication between them and bodies with open pores; let the one touch the bar, and the other the metallic mass, while the machine is put in motion: it is evident that the former will become positively electric, or surcharged with ether; whereas the other, he who touches the metallic mass, will acquire a negative electricity, and lose his ether.

Here, then, are two electric men, but in a manner totally different, though rendered such by the same machine. Both will be surrounded by an electric atmosphere, which in the dark will appear like the light that painters throw about the figures of saints. The reason is, that the superfluous ether of the one insensibly escapes into the circumambient air; and that, with respect to the other, the ether contained in the air insensibly insinuates itself into his body. This transition, though insensible, will be accompanied with an agitation of ether, which produces light.

It is evident that these two species of electricity are directly opposite; but in order to have a thorough conviction of it, let these two join hands, or only touch each other, and you will see very vehement sparks issue from their bodies, and they themselves will feel very acute pain.

If they were electrified in the same manner, which would be the case if both touched the bar or the metallic mass, they might safely touch each other; no spark and no pain would ensue, because the ether contained in both would be in the same state; whereas, in the case laid down, their state is directly opposite.

25th July, 1761.

LETTER XXXIV.

The Leyden Experiment.

I NOW proceed to describe a phenomenon of electricity which has made a great deal of noise, and which is known by the name of *the Leyden experiment*, because *Mr. Muschenbroeck*, professor at Leyden, is the inventor of it.[*] What is most astonishing in this experiment is the terrible stroke resulting from it, by which several persons at once may receive a very violent shock.

Let C, *Fig*. 99, be a globe of glass, turned round by means of the handle E, and rubbed by the cushion D D, which is pressed upon the globe by the spring O. At Q are the metallic filaments which transmit the electricity into the bar of iron F G, by the metallic chain P.

Hitherto there is nothing different from the process already described. But in order to execute the

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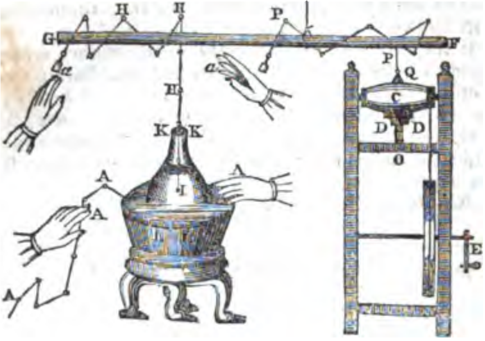
[*] The first person who witnessed the shock was Cuneus, a clergyman of Leyden. Holding a tumbler of water in one hand, he allowed a stream of electric fluid to pass into the water through a wire, which hung from the prime conductor, to ascertain its effects upon the taste of the water: When he thought the water sufficiently electrified, he was about to remove the wire with his other hand; and, on touching it, to his great astonishment, received the shock.—*Am. Ed.*

Fig. 99.



experiment in question, to the bar is attached another chain of metal H, one extremity of which I is introduced into a glass bottle K K, filled with water; the bottle too is placed in a basin L L, likewise filled with water. You plunge at pleasure into the water in the basin another chain A, one end of which drags on the floor.

Having put the machine in motion for some time, that the bar may become sufficiently electric, you know that if the finger were to be presented to the extremity of the bar at *a*, the usual stroke of electricity would be felt from the spark issuing out of it. But were the same person at the same time to put the other hand into the water in the basin at A, or were he but to touch with any part of his body the chain plunged in that water, he would receive a stroke incomparably more violent, by which his whole frame would undergo a severe agitation.

This shock may be communicated to many persons at once. They have only to join hands, or to touch each other, were it but by the clothes; then the first puts his hand into the water in the basin, or touches the chain only, one end of which is plunged into it; and as soon as the last person applies his finger to the bar you will see a spark dart from it much more vehement than usual, and the whole chain of persons feel, at the same instant, a very violent shock over their whole body.

Such is the famous Leyden experiment, which is so much the more surprising, that it is difficult to see how the bottle and the water in the basin contribute to increase so considerably the effect of the electricity. To solve this difficulty, permit me to make the following reflections.

1. While by the action of the machine the ether is compressed in the bar, it passes by the chain H into the water contained in the bottle I, and there meeting a body with open pores, the water in the bottle will become as much surcharged with ether as the bar itself.

2. The bottle, being glass, has its pores close, and therefore does not permit the ether compressed within it to pierce through the substance of the glass, to discharge itself into the water in the basin; consequently, the water in the basin remains in its natural state, and will not become electric; or even on the supposition that a little of the ether might force its way through the glass, it would presently be lost in the basin and pedestal, the pores of which are open.

3. Let us now consider the case of a man with one hand in the water in the basin, or only in contact with the chain A, one extremity of which is immersed in that water; let him present the other hand to the bar at *a*, the result will be as the first effect, that with the spark which issues from the bar the ether will make its escape with great velocity, and meeting everywhere in the body of the man open pores, will without obstruction be diffused over it.

4. Hitherto we see only the usual effect of elec-

tricity; but while the ether with such rapidity flies over the body of the man, it discharges itself with equal rapidity, by the other hand, or by the chain A, into the water in the basin; and as it enters this with such impetuosity, it will easily overcome the obstacle opposed by the glass, and penetrate into the water which the bottle contains.

5. Now the water in the bottle containing already an ether too much compressed, it will acquire from this increase new force, and will diffuse itself with impetuosity, as well through the chain I H as through the bar itself: it will of consequence make its escape thence at *a* with new efforts; and as this is performed in an instant, it will enter into the finger with increased force to be diffused over the whole body of the man.

6. Passing thence anew into the water in the basin, and penetrating the bottle, it will increase still further the agitation of the ether compressed in the water of the bottle and in the bar; and this will continue till the whole is restored to equilibrium, which will quickly take place, from the great rapidity with which the ether acts.

7. The same thing will happen if you employ several persons instead of one man. And now I flatter myself, you fully comprehend whence arises the surprising increase of force in the electricity which is produced by this experiment of *Mr. Muschenbroeck*, and which exhibits effects so prodigious.

8. If any doubt could remain respecting what I have advanced, that the ether compressed in the water of the bottle could not penetrate through the glass, and that afterward I have allowed it a passage abundantly free—such doubt will vanish when it is considered, that in the first case every thing is in a state of tranquillity, and in the last the ether is in a terrible agitation, which must undoubtedly assist its progress through the closest passages.

28th July, 1761.

LETTER XXXV.

Reflections on the Cause and Nature of Electricity, and on other Means proper to produce it.

After these elucidations, you can be at no loss respecting the cause of the prodigious effects observable in the phenomena of electricity.

Most authors who have treated the subject, perplex the experiments in such a manner that they are rendered absolutely unintelligible, especially when they attempt an explanation. They have recourse to a certain subtile matter, which they denominate *the electric fluid*, and to which they ascribe qualities so extravagant, that the mind rejects them with contempt; and they are constrained to acknowledge, at length, that all their efforts are insufficient to furnish us with a solid knowledge of these important phenomena of nature.

But you are enabled to conclude, from the principles which I have unfolded, that bodies evidently become electric only so far as the elasticity, or the state of the compression of the ether in the pores of bodies, is not the same as everywhere else; in other words, when it is more or less compressed in some than in others. For in that case the prodigious elasticity with which the ether is endowed makes violent efforts to recover its equilibrium, and to restore everywhere the same degree of elasticity, as far as the nature of the pores, which in different bodies are more or less open, will permit; and it is the return to equilibrium which always produces the phenomena of electricity.

When the ether escapes from a body where it is more compressed, to discharge itself into another where it is less so, this passage is always obstructed by the close pores of the air; hence it is put into a

certain agitation, or violent motion of vibration, in which, as we have seen, light consists; and the more violent this motion is, the more brilliant the light becomes, till it is at length capable of setting bodies on fire, and of burning them.

While the ether penetrates the air with so much force, the particles of air are likewise put into a motion of vibration, which occasions sound; it is accordingly observed, that the phenomena of electricity are accompanied with a cracking noise, greater or less, according to the diversity of circumstances.

And as the bodies of men and animals are filled with ether in their minutest pores, and as the action of the nerves seems to depend on the ether contained in them, it is impossible that men and animals should be indifferent with respect to electricity: and when the ether in them is put into a great agitation, the effect must be very sensible, and, according to circumstances, sometimes salutary, and sometimes hurtful. To this last class, undoubtedly, must be referred the terrible shock of the Leyden experiment; and there is every reason to believe that it might be carried to a degree of force capable of killing men, for by means of it many small animals, such as mice and birds, have actually been killed.

Though friction usually is employed in the production of electricity, you will easily comprehend that there may be other means besides this. Whatever is capable of carrying the ether contained in the pores of a body to a greater or less degree of compression than ordinary, renders it electric: and if its pores are close, there the electricity will be of some duration; whereas, in bodies whose pores are open it cannot possibly subsist, unless surrounded by air, or other bodies with close pores.

Hence it has been observed, that heat frequently supplies the place of friction. When you heat or melt sealing-wax or sulphur in a spoon, you discover a very sensible electricity in these substances after they are cooled. The reason is no longer a mystery, as we know that heat enlarges the pores of all bodies, for they occupy a greater space when hot than when they are cold.

You know that in a thermometer the mercury rises in heat and falls in cold; because it occupies a greater space when it is hot, and fills the tube more than when it is cold. We find, for the same reason, that a bar of iron very hot is always somewhat longer than when cold—a property common to all bodies with which we are acquainted.

When, therefore, we melt by fire a mass of sealing-wax or sulphur, their pores are enlarged, and probably more open; a greater quantity of ether must of course be introduced to fill them. When, afterward, these substances are suffered to cool, the pores contract and close, so that the ether in them is reduced to a smaller space, and consequently carried to a higher degree of compression, which increases its elasticity: these masses will acquire, therefore, a positive electricity, and must consequently exhibit the effects of it.

This property of becoming electric by heat is remarked in most precious stones. Nay, there is a stone named *tourmaline*, which, when rubbed or heated, acquires at once the two species of electricity. The ether in one part of the stone is expelled to compress more that which is in the other part; and its pores are too close to permit the re-establishment of the equilibrium.*

1*st August*, 1761.

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* Very important discoveries have been made since the time of Euler, respecting the production of electricity by *friction*, *pressure*, and *heat*. A very brief notice of these will be interesting to the reader.

Electricity by Friction.—Rock crystal, and almost all the precious stones, acquire *positive* or *vitreous* electricity with whatever substances they are rubbed; and on the other hand, resin, sulphur, bitumen, &c. acquire negative or resinous electricity when rubbed with any non-conducting substance. Glass, however, when polished, gives vitreous electricity by friction, whereas it gives resinous electricity when it is rough. Among the metals *zinc* and *bismuth* always acquire vitreous electri-

LETTER XXXVI.

Nature of Thunder: Explanations of the Ancient Philosophers, and of Descartes: Resemblance of the Phenomena of Thunder to those of Electricity.

I HAVE hitherto considered electricity only as an object of curiosity and speculation to naturalists; but

city when rubbed with a piece of woollen cloth, while *tin* and *antimony* always acquire resinous electricity. In many of the other metals, and in various other substances, the results are often irregular and anomalous, sometimes one kind of electricity being developed, and sometimes another. The most striking example of this is in the mineral called *kyanite*, some crystals of which always acquire resinous electricity by friction, while other crystals always acquire vitreous electricity. In some of these crystals, indeed, vitreous electricity is obtained by rubbing one face, and resinous electricity by rubbing the other. For further information on this subject, see Haüy's *Traité de Mineralogie*, Paris, 1822, vol. i. p. 186; and the *Edinburgh Encyclopædia*, Article ELECTRICITY, vol. viii. p. 430.

Electricity by Pressure.—The Abbé Haüy discovered the method of producing electricity by pressure. He found that if a rhomb of Iceland spar is held in one hand by two of its opposite edges, and if with two fingers of the other hand two of its opposite faces are merely *touched*, it gives out vitreous electricity. When pressure is applied in place of contact, the effect is greatly increased. Haüy found the same property in topaz, euclase, arragonite, fluate of lime, carbonate of lead, and hyalin quartz, all of which give vitreous electricity, both by friction and pressure. Sulphate of barytes and sulphate of lime give no electricity by pressure. Elastic bitumen, which is negatively electrified by friction, is also negatively electrified by pressure.

Electricity by Heat.—The property possessed by *tourmaline* of becoming electrical by heat seems to have been known to the ancients. When tourmaline, oxide of zinc, borate of magnesia, Auvergne mesotype, Greenland mesotype, scolezite, and mesolite, are heated, one extremity of the crystal developes resinous and the other vitreous electricity, the intensity of electricity diminishing rapidly from the extremities to the middle or neutral point of the crystal. In the boracite there are eight electrical poles, one at each solid angle of the cube.

When these minerals again reach the ordinary temperature, the electricity disappears; but M. Haüy has lately found, that it then passes by a reduction of temperature to the opposite state. With oxide of zinc and tourmaline he invariably found, that the opposite electricity could be developed by cold, so that the pole which possessed vitreous electricity when it was hot developed resinous electricity when it was cold. When the opposite electricity is beginning to show itself, the two poles have you will presently see, not without some degree of surprise, that thunder and lightning, as well as all the terrible phenomena which accompany them, derive their origin from the same principle; and that in these nature executes on the great scale what naturalists do in miniature by their experiments.

Those philosophers who thought they saw some resemblance between the phenomena of thunder and those of electricity were at first considered as visionaries; and it was imagined that they made use of this pretence merely as a cover to their ignorance respecting the cause of thunder: but you will soon be convinced that every other explanation of these grand operations of nature is destitute of foundation.

In truth, every thing advanced on the subject previous to the knowledge of electricity was a mass of absurdity, and little calculated to convey instruction respecting any of the phenomena of thunder.

Ancient philosophers attributed the cause of it to

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sometimes at once both vitreous and resinous electricity. The disappearance of the opposite electricity produced by cold takes place generally below the freezing point.—See Haüy's *Traité de Mineralogie*, tom. 1. p. 200.

In examining the electricity of the tourmaline, I have found that it may be shown in a very satisfactory manner, by means of a thin slice taken from any part of the prism, and having its surfaces perpendicular to the axis of the prism. It must then be placed upon a piece of well polished glass, and the glass heated to a considerable degree. At the proper temperature, which is about that of boiling water, the slice will adhere to the glass so firmly, that even when the glass is above the tourmaline, the latter will adhere to it for six or eight hours. By this means slices of a very considerable breadth and thickness develop as much electricity as is capable of supporting their own weight. The slice of tourmaline adheres equally to all bodies whatever. Mr. Sivright has fitted up a tourmaline so as to bring the action of its two poles very near to one another. It resembles the letter D, with an opening in its curved part. The straight part is the tourmaline, and the two curved parts are pieces of silver wire rising out of two silver caps; one of which embraces each pole of the tourmaline. A pith ball suspended at the opening vibrates between the extremities of the wires. Sir H. Davy (*Elements of Chymical Philosophy*, vol. i. p. 130) states, that when the slice is of considerable size, flashes of light may be seen along its surface.—See *Edinburgh Philosophical Journal*, vol. i. p. 208.—*Ed.*

sulphureous and bituminous vapours, which, ascending from the earth into the air, mixed with the clouds, where they caught fire from some unknown cause.

Descartes, who quickly perceived the insufficiency of this explanation, imagined another cause in the clouds themselves, and thought that thunder might be produced by the sudden fall of more elevated clouds on others in a lower region of the air; that the air contained in the intermediate space was compressed by this fall to such a degree as was capable of exciting a noise so loud, and even of producing lightning and thunder, though it was impossible for him to demonstrate the possibility of it.

But without fixing your attention on false explanations, which lead to nothing, I hasten to inform you that it has been discovered by incontestable proofs, that the phenomena of thunder are always accompanied by the most indubitable marks of electricity.

Let a bar of metal, say of iron, be placed on a pillar of glass, or any other substance whose pores are close, that when the bar acquires electricity it may not escape or communicate itself to the body which supports the bar; as soon as a thunder-storm arises, and the clouds which contain the thunder come directly over the bar, you perceive in it a very strong electricity, generally far surpassing that which art produces; if you apply the hand to it, or any other body with open pores, you see bursting from it, not only a spark, but a very bright flash, with a noise similar to thunder; the man who applies his hand to it receives a shock so violent that he is stunned. This surpasses curiosity; and there is good reason why we should be on our guard and not approach the bar during a storm.

A professor at Petersburg, named *Richmann*, has furnished a melancholy example. Having perceived a resemblance so striking between the phenomena of thunder and those of electricity, this unfortunate naturalist, the more clearly to ascertain it by experiment, raised a bar of iron on the roof of his house, cased below in a tube of glass, and supported by a mass of pitch. To the bar he attached a wire, which he conducted into his chamber, that as soon as the bar should become electric, the electricity might have a free communication with the wire, and so enable him to prove the effects in his apartment. And it may be proper to inform you, that this wire was conducted in such a manner as nowhere to be in contact but with bodies whose pores are close, such as glass, pitch, or silk, to prevent the escape of the electricity.

Having made this arrangement, he expected a thunder-storm, which, unhappily for him, soon came. The thunder was heard at a distance: *Mr. Richmann* was all attention to his wire, to see if he could perceive any mark of electricity. As the storm approached, he judged it prudent to employ some precaution, and not keep too near the wire; but happening carelessly to advance his chest a little, he received a terrible stroke, accompanied with a loud clap, which stretched him lifeless on the floor.

About the same time, the late *Dr. Lieberkuhn* and *Dr. Ludolf* were preparing to make similar experiments in this city, and with that view had fixed bars of iron on their houses; but being informed of the disaster which had befallen *Mr. Richmann*, they had the bars of iron immediately removed; and, in my opinion, they acted wisely.

From this you will readily judge, that the air or atmosphere must become very electric during a thunder-storm, or that the ether contained in it must then be carried to a very high degree of compression. This ether, with which the air is surcharged, will pass into the bar, because of its open pores;

and it will become electric, as it would have been in the common method, but in a much higher degree.

4th August, 1761.

LETTER XXXVII.

Explanation of the Phenomena of Lightning and Thunder.

The experiments now mentioned incontestably demonstrate, therefore, that stormy clouds are extremely electrical, and that consequently their pores are either surcharged with ether, or exhausted, as both states are equally adapted to electricity. But I have very powerful reasons for believing that this electricity is positive, that the ether in them is compressed to the highest degree, and consequently so much the more elastic than elsewhere.

Such storms usually succeed very sultry weather. The pores of the air, and of the vapours floating in it, are then extremely enlarged, and filled with a prodigious quantity of ether, which easily takes possession of all the empty spaces of other substances. But when the vapours collect in the superior regions of our atmosphere, to form clouds, they have to encounter excessive cold. Of this it is impossible to doubt, from the hail which is frequently formed in these regions: this is a sufficient proof of a congelation, as well as the snow which we find on the tops of very high mountains, such as the Cordilleras, while extreme heat prevails below.

Nothing then is more certain, or better established by proof, than the excessive cold which universally prevails in the upper regions of the atmosphere, where clouds are formed. It is equally certain, that cold contracts the pores of bodies, by reducing them to a smaller size: now, as the pores of the vapours have been extremely enlarged by the heat, as soon as they are formed aloft into clouds, the pores contract, and the ether which filled them, not being able to escape, because those of the air are very close, it must needs remain there: it will be of course compressed to a much higher degree of density, and consequently its elasticity will be so much the greater.

The real state of stormy clouds, then, is this—the ether contained in their pores is much more elastic than usual, or, in other words, the clouds have a positive electricity. As they are only an assemblage of humid vapours, their pores are very open; but being surrounded by the air, whose pores are close, this ether could not escape but very imperceptibly. But if any person, or any body whatever with open pores, were to approach it, the same phenomena which electricity exhibits would present themselves; a very vehement spark, or rather a real flash, would burst forth. Nay more, the body would undergo a very violent shock by the discharge, from the impetuosity with which the ether in the cloud would rush into its pores. This shock might be indeed so violent as to destroy the structure; and, finally, the terrible agitation of the ether which bursts from the cloud, being not only light, but a real fire, it might be capable of kindling and consuming combustible bodies.

Here, then, you will distinguish all the circumstances which accompany thunder; and as to the noise of thunder, the cause is very obvious, for it is impossible the ether should be in such a state of agitation without the air itself receiving from it the most violent concussions, which forcibly impel the particles, and excite a dreadful noise. Thunder, then, bursts forth as often as the force of ether contained in the clouds is capable of penetrating into a body where the ether is in its natural state, and whose pores are open: it is not even neces-

sary that such body should immediately touch the cloud.

What I have said respecting the atmosphere of electrified bodies principally takes place in clouds; and frequently, during a storm, we are made sensible of this electric atmosphere by a stifling air, which is particularly oppressive to certain persons. As soon as the cloud begins to dissolve into rain, the air, becoming humid by it, is charged with an electricity, by which the commotion may be conveyed to bodies at a very great distance.

It is observed that thunder usually strikes very elevated bodies, such as the summits of church-spires, when they consist of substances with open pores, as all metals are; and the pointed form contributes not a little to it. Thunder frequently falls likewise on water, the pores of which are very open; but bodies with close pores, as glass, pitch, sulphur, and silk, are not greatly susceptible of the thunder-stroke, unless they are very much moistened. It has been accordingly observed, that when thunder passes through a window, it does not perforate the glass, but always the lead or other substances which unite the panes. It is almost certain, that an apartment of glass cemented by pitch, or any other substance with close pores, would be an effectual security against the ravages of thunder.

8th August, 1761.

LETTER XXXVIII.

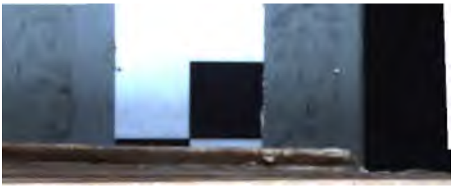
Continuation.

Thunder, then, is nothing else but the effect of the electricity with which the clouds are endowed; and as an electrified body, applied to another in its natural state, emits a spark with some noise, and discharges into it the superfluous ether with prodigious impetuosity, the same thing takes place in a cloud that is electric, or surcharged with ether, but with a force incomparably greater, because of the terrible mass that is electrified, and in which, according to every appearance, the ether is reduced to a much higher degree of compression than we are capable of producing in it by our machinery.

When, therefore, such a cloud approaches bodies prepared for the admission of its ether, this discharge must be made with incredible violence: instead of a simple spark, the air will be penetrated with a prodigious flash, which, exciting a commotion in the ether contained in the whole adjoining region of the atmosphere, produces a most brilliant light; and in this lightning consists.

The air is at the same time put into a very violent motion of vibration, from which results the noise of thunder. This noise must, no doubt, be excited at the same instant with the lightning; but you know that sound always requires a certain quantity of time, in order to its transmission to any distance, and that its progress is only at the rate of about eleven hundred feet in a second; whereas light travels with a velocity inconceivably greater. Hence we always hear the thunder later than we see the lightning; and from the number of seconds intervening between the flash and the report, we are enabled to determine the distance of the place where it is generated, allowing eleven hundred feet to a second.

The body itself, into which the electricity of the cloud is discharged, receives from it a most dreadful stroke; sometimes it is shivered to pieces—sometimes set on fire and consumed, if combustible—sometimes melted, if it be of metal; and, in such cases, we say it is thunder-struck, the effects of which, however surprising and extraordinary they may appear, are in perfect consistency with the well-known phenomena of electricity.



A sword, it is known, has sometimes been by thunder melted in the scabbard, while the last sustained no injury: this is to be accounted for from the openness of the pores of the metal, which the ether very easily penetrates, and exercises over it all its powers; whereas the substance of the scabbard is more closely allied to the nature of bodies with close pores, which do not permit the ether so free a transmission.

It has likewise been found, that of several persons on whom the thunder has fallen, some only have been struck by it; and that those who were in the middle suffered no injury. The cause of this phenomenon likewise is manifest. In a group exposed to a thunder-storm, they are in the greatest danger who stand in the nearest vicinity to the air that is surcharged with ether; as soon as the ether is discharged upon one, all the adjoining air is brought back to its natural state, and consequently those who were nearest to the unfortunate victim feel no effect; while others, at a greater distance, where the air is still sufficiently surcharged with ether, are struck with the same thunder-clap.

In a word, all the strange circumstances so frequently related of the effects of thunder contain nothing which may not be easily reconciled with the nature of electricity.

Some philosophers have maintained, that thunder does not come from the clouds, but from the earth, or from bodies. However extravagant this sentiment may appear, it is not so absurd, as it is difficult to distinguish in the phenomena of electricity whether the spark issues from the body which is electrified, or from that which is not so, as it equally fills the space between the two bodies; and if the electricity is negative, the ether and the spark are in reality emitted from the natural or non-electrified body. But we are sufficiently assured, that in thunder the clouds have a positive electricity, and that the lightning is emitted from the clouds.

You will be justifiable, however, in asking, if by every stroke of thunder some terrestrial body is affected? We see, in fact, that it very rarely strikes buildings, or the human body; but we know, at the same time, that trees are frequently affected by it, and that many thunder-strokes are discharged into the earth and into the water. I believe, however, it might be maintained, that a great many do not descend so low, and that the electricity of the clouds is very frequently discharged into the air or atmosphere.

The small opening of the pores of the air no longer opposes any obstruction to it, when vapours or rain have rendered it sufficiently humid; for then we know the pores are open.

It may very possibly happen in this case, that the superfluous ether of the clouds should be discharged simply into the air; and when this takes place, the strokes are neither so violent, nor accompanied with so great a noise, as when the thunder bursts on the earth, when a much greater extent of atmosphere is put in agitation.

11th August, 1761.

LETTER XXXIX.

The Possibility of preventing, and of averting, the Effects of Thunder.

It has been asked, Whether it might not be possible to prevent or to avert the fatal effects of thunder? You are well aware of the importance of the question, and under what obligation I should lay a multitude of worthy people, were I able to indicate an infallible method of finding protection against thunder.

The knowledge of the nature and effects of electricity permits me not to doubt that the thing is possible. I corresponded some time ago with a Moravian priest, named *Procopius Divisch*, who assured me that he had averted, during a whole summer, every thunder-storm which threatened his own habitation and the neighbourhood, by means of a machine constructed on the principles of electricity. Several persons since arrived from that country have assured me that the fact is undoubted, and confirmed by irresistible proof.

But there are many respectable characters who, on the supposition that the thing is practicable, would have their scruples respecting the lawfulness of employing such a preservative. The ancient pagans, no doubt, would have considered him as impious who should have presumed to interfere with Jupiter in the direction of his thunder. Christians, who are assured that thunder is the work of God, and that Divine Providence frequently employs it to punish the wickedness of men, might with equal reason allege that it were impiety to attempt to oppose the course of sovereign justice.

Without involving myself in this delicate discussion, I remark that conflagrations, deluges, and many other general calamities are likewise the means employed by Providence to punish the sins of men; but no one surely ever will pretend, that it is unlawful to prevent or resist the progress of a fire or an inundation. Hence I infer, that it is perfectly lawful to use the means of prevention against the effects of thunder, if they are attainable.

The melancholy accident which befell *Mr. Richmann* at Petersburg demonstrates that the thunder-stroke which this gentleman unhappily attracted to himself, would undoubtedly have fallen somewhere else, and that this place thereby escaped; it can therefore no longer remain a question whether it be possible to conduct thunder to one place in preference to another; and this seems to bring us near our mark.

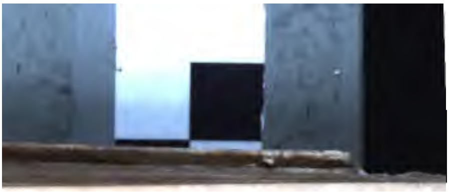
It would no doubt be a matter of still greater importance to have it in our power to divest the clouds of their electric force, without being under the necessity of exposing any one place to the ravages of thunder; we should in that case altogether prevent these dreadful effects, which terrify so great a part of mankind.

This appears by no means impossible; and the Moravian priest whom I mentioned above unquestionably effected it; for I have been assured that his machinery sensibly attracted the clouds, and constrained them to descend quietly in a distillation, without any but a very distant thunder-clap.

The experiment of a bar of iron, in a very elevated situation, which becomes electric on the approach of a thunder-storm, may lead us to the construction of a similar machine, as it is certain that in proportion as the bar discharges its electricity the clouds must lose precisely the same quantity; but it must be contrived in such a manner, that the bars may immediately discharge the ether which they have attracted.

It would be necessary for this purpose to procure for them a free communication with a pool, or with the bowels of the earth, which, by means of their open pores, may easily receive a much greater quantity of ether, and disperse it over the whole immense extent of the earth, so that the compression of the ether may not become sensible in any particular spot. This communication is very easy, by means of chains of iron, or any other metal, which will with great rapidity carry off the ether with which the bars are surcharged.

I would advise the fixing of strong bars of iron, in very elevated situations, and several of them together, their higher extremity to terminate in a point, as this figure is very much adapted to the



attraction of electricity. I would afterward attach long chains of iron to these bars, which I would conduct under ground into a pool, lake, or river, there to discharge the electricity; and I have no doubt, that after making repeated essays, the means may be certainly discovered of rendering such machinery more commodious, and more certain in its effect.[*]

It is abundantly evident, that on the approach of a thunder-storm, the ether with which the clouds are surcharged would be transmitted in great abundance into these bars, which would thereby become very electric, unless the chains furnished to the ether a free passage, to spend itself in the water and in the bowels of the earth.

The ether of the clouds would continue, therefore, to enter quietly into the bars, and would by its agitation produce a light which might be visible on the pointed extremities.

Such light is, accordingly, often observed during a storm on the summit of spires—an infallible proof that the ether of the cloud is there quietly discharging itself; and every one considers this as a very good sign of the harmless absorption of many thunder-strokes.

Lights are likewise frequently observed at sea on the tops of the masts of ships, known to sailors by the name of *Castor and Pollux*;[†] and when such signs are visible, they consider themselves as safe from the stroke of thunder.

Most philosophers have ranked these phenomena among vulgar superstitions; but we are now fully assured that such sentiments are not without foundation; indeed, they are infinitely better founded than many of our philosophical reveries.*

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[*] As buildings are often struck laterally, the main thunder-rod, especially in monumental pillars and elevated buildings, should have various lateral branches diverging from it, and extending to the air through openings in the building. By this means it is secured much more effectually than when there is only one conductor, which can do no more than protect the summit of the building.—*Ed.*

[†] This phenomenon is also called the Fire of St. Elmo. A very interesting account of it will be found in the *Edinburgh Philosophical Journal*, vol. ix. p. 35.—*Ed.*

15th August, 1761.

LETTER XL.

On the celebrated Problem of the Longitude: General Description of the Earth, of its Axis, its two Poles, and the Equator.

You will by this time, no doubt, imagine that enough has been said of electricity; and indeed I have nothing further to add on that subject; and am, of course, not a little embarrassed about the choice of one worthy of your attention.

In order to determine my choice, I think myself obliged to take into consideration those subjects which most materially interest human knowledge, and which authors of celebrity most frequently bring

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* A very copious account of the recent discoveries in electricity will be found in the article on that subject, in the *Edinburgh Encyclopædia*, vol. viii. p. 411.—*Ed.* [It is remarkable that neither Euler nor his European editor have anywhere noticed the discoveries of Dr. Franklin, admitted as they are, almost universally, to lie at the foundation of the most intelligible principles of the science, and to have enriched it with the most useful facts. The omission is the more surprising, since the experiments of the American philosopher which demonstrated the identity of lightning and the fluid of an electrical machine were made in 1752, nine years prior to the date of Euler's Letter; and that his letters to Peter Collinson, of London, describing his experiments and discoveries, were published in almost all the languages of Europe, and more eagerly read than any thing that had appeared on that new and interesting subject. To Dr. Franklin the world is certainly indebted for the application of the rod to the preservation of buildings. His views also of the nature of electrical agency were cordially received by the scientific world, and still constitute the basis of the prevailing theory,—while that of Euler never attained much vogue among the learned, and is now no longer heard of. So prominent a station does Dr. Franklin hold among the most successful cultivators of this science, and so numerous are the facts which have been added since his day, we can only refer the reader to Dr. Priestley's History of Electricity, and to some good modern treatises on the subject.—*Am. Ed.*]

forward. These are subjects respecting which, it is to be presumed, persons of quality have considerable information.

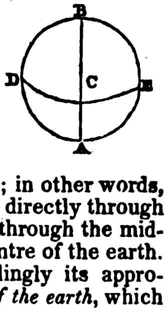
As you must unquestionably have heard frequent mention made of the celebrated problem of the longitude, for the solution of which the British nation has proposed a most magnificent premium, I presume that my labour will not be wholly thrown away if I employ it in laying before you a fair state of that important question. It has such an intimate connexion with the knowledge of our terraqueous globe, that it were a shame to be ignorant of it. It will accordingly furnish me with an opportunity of explaining a variety of interesting articles, which I flatter myself you would wish to see elucidated.

I begin, then, with a general description of the earth, which may be considered as a globe, though it has been discovered by recent observation that its real figure is a spheroid somewhat flattened; but the difference is so small that it may for the present be altogether neglected.

The first thing to be remarked on the globe of the earth are two points on its surface denominated the two poles of the earth. Round these two points the globe of the earth every day revolves, as you turn a ball fixed between the two points of a turning machine. This motion is called the daily or diurnal motion of the earth, each revolution of which is performed in about twenty-four hours; or, to speak according to appearances, you know that the whole heavens, which we consider as a concave ball, within whose circumference the earth revolves, appear to turn round the earth in the same space of twenty-four hours. This motion is likewise performed round two fixed points in the heavens, denominated the poles of heaven; now if we conceive a straight line drawn from one of these poles of heaven to the other, that line will pass through the centre of the earth.

But you will easily comprehend that the appearance must be the same, whether the earth turns round these poles while the heavens remain in a state of rest; or whether the heavens revolve round their poles, the earth remaining at rest. On either supposition we are equally led to the knowledge of the poles of the earth, the foundation not only of astronomy, but likewise of geography.

Fig. 100.



Let *Fig*. 100 represent the globe of the earth, whose poles are at the points A and B; one of these poles, A, is named the *south* or *antarctic pole*, the other, B, is denominated the *north* or *arctic pole*. This last is nearer to the region of the globe which we inhabit.

I remark that these two poles are directly opposite to each other; in other words, were a straight line A B to be drawn directly through the earth, it would pass precisely through the middle C, that is to say, through the centre of the earth. This straight line A B has accordingly its appropriate name, and is called *the axis of the earth*, which being produced in both directions to the heavens, will terminate in the two points which are called the poles of heaven; and to which we give the same names as to those of the earth.

These two poles of the earth are by no means a mere fiction, or a speculation of astronomers and geographers; but are really most essential points marked on the surface of our globe; for it is well known, that the nearer we approach these two points, the colder* and more rugged the face of nature be-

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* I have lately had occasion to show, that the greatest cold is not at the poles, but at two points on each side of the pole, nearly coincident with the magnetic poles. The mean temperature of Melville Island, which Captain Parry found to be 1⅓° for 1819-1820, is undoubtedly lower than that of the north pole of our globe.—See *Edinburgh Transactions*, vol ix. p. 201.—*Ed.*

comes, to such a degree that the regions adjacent to the poles are absolutely uninhabitable, from the excessive cold which prevails there during the winter. No one instance, accordingly, has been produced of any traveller, whether by land or water, who has reached either of the poles. It may be affirmed, therefore, that these two spots of the earth are altogether inaccessible.

Having thus determined the two poles of the earth A and B, we may conceive the whole globe divided into two hemispheres, D B E and D A E, each of which terminates in one of the poles as its summit. For this purpose we are to suppose the globe bisected through its centre C, so that the section shall be perpendicular to the axis of the earth; this section will mark on the surface a circle encompassing the whole globe, everywhere equally distant from the two poles. This surrounding circle is denominated the *equator*. The regions adjacent to it are the hottest, and on that account, as the ancients believed, almost uninhabitable; but they are now found to be exceedingly populous, though the heat be there almost insupportable.

But as you remove from the equator on either side towards the poles, the countries becomes more and more temperate, till at last, on approaching too near the poles, the cold becomes intolerable.

As the equator divides the earth into two hemispheres, each bears the name of the pole contained in it; thus the half D B E, which contains the north pole, is denominated the *northern hemisphere*, and in it is situated all Europe, almost the whole of Asia, part of Africa, and the half of America. The other hemisphere, D A E, is from its pole denominated the *southern hemisphere*, and contains the greater part of Africa, the other half of America, and several isles, which geographers attribute to Asia, as you will recollect to have seen in maps of the world.

18th August, 1761.

LETTER XLI.

Of the Magnitude of the Earth; of Meridians, and the shortest Road from Place to Place.

HAVING distinctly fixed the idea of the poles of the earth and of the equator, which you much more easily imagine on a globe than I can represent by a figure, every other necessary idea will readily follow from these.

I must, however, subjoin a further elucidation of considerable importance. The axis of the earth, passing from one pole to the other through the centre of the earth, is a diameter of the globe, and consequently is double the length of the radius. A radius of the earth, or the distance from every point on the surface to the centre, is computed to be 3956 English miles; the axis of the earth will therefore contain 7912 English miles. And the equator being a circle whose centre is likewise that of the earth, it will have nearly the same radius, namely 3956 miles; the diameter of the equator will accordingly be 7912 miles, and its whole circumference 23,736 miles nearly: so that if you were to make a tour of the globe, following the track of the equator, you must perform a journey of almost 23,736 English miles. This will give you some idea of the magnitude of the earth.

The equator being a circle, it is supposed to be divided into 360 equal parts, named *degrees;* a degree of the equator contains, therefore, 65 English miles,[*] as 9 times 360 make nearly 24,196.[†]

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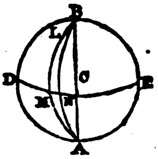
[*] These results are only approximative. As the earth is a spheroid, flattened at the poles like an orange, the circumference of the meridian is about 24,855.84 English miles, and the circumference of the equator 24,896.16 English miles. A geographical mile of 60 to a degree will therefore contain 6075.6 English feet.—*Ed.*

[†] This paragraph, as it stands, is unintelligible. A degree at the equator is about 69.2 × 360 = 24,912, the circumference of the earth. The numbers in the preceding paragraph are still more erroneous.—*Am. Ed*

Every degree is again subdivided into 60 equal parts, called *minutes*, so that every minute contains more than an English mile, or 6076 English feet; a *second*, being the sixtieth part of a minute, will contain 101 English feet.

Fig. 101.



It being impossible to represent a globe on paper any other way than by a circle, you must supply this defect by imagination. Accordingly, A B, *Fig.* 101, being the two poles of the earth, B the north, and A the south, D M N E will represent the equator, or rather that half of it which is turned towards us, the other being concealed on the opposite side.

The line D M N E represents, then, a semicircle, as well as B E A and B D A; all these semicircles having their centres at that of the globe C. It is possible to imagine an infinite number of other semicircles, all of them drawn through the two poles of the earth A and B, and passing through every point of the equator, as B M A, B N A; these will all be similar to the first, B D A and B E A, though in the figure their form appears very different. Imagination must correct this, and the fact is apparent on a real globe.

All these semicircles drawn from one pole to the other, through whatever point of the equator they may pass, are denominated *meridians;* or rather, a *meridian* is nothing else but a semicircle, which on the surface of the earth is drawn from one pole to the other; and you can easily comprehend that taking any place whatever on the surface of the earth, say the point L, you can always conceive a meridian B L M A, which passing through the two poles takes in its way the point L. This meridian, then, is named the *meridian of* L. Supposing, for example, L to be Berlin, the semicircle B L M A would be the meridian of Berlin; and the same may be said respecting every other spot of the earth.

You can represent to yourself a globe, on the surface of which are described all the countries of the earth, the continent, as well as the sea, with its islands. This artificial globe, denominated the *terrestrial* or *terraqueous globe*, you must no doubt be acquainted with. As to all meridians which can possibly be drawn upon it, and a great number of which actually are traced, I remark, that each being a semicircle is divided by the equator into two equal parts, each of which is the fourth part of a circle, that is, an arch of 90 degrees. Accordingly, B D, B M, B N, B E, are fourth parts of a circle, as well as A D, A M, A N, A E; each therefore contains 90 degrees: and it may be further added, that each is perpendicular to the equator, or forms right angles with it.

Again, were a person to travel from the point of the equator M to the pole B, the shortest road would be to pursue the track of the meridian M L B, which being an arch of 90 degrees, will contain 6214 English miles; the distance to be passed in going from the equator to either of the poles.

You will recollect that the shortest road from place to place is the straight line drawn through any two places; here the straight line drawn from the point M in the equator to the pole B would fall within the earth—a route which it is impossible to pursue, for we are so attached to the surface of the earth that we cannot remove from it. For this reason, the question becomes exceedingly different when it is asked, What is the shortest road leading from one spot on the surface of a globe to another? This shortest road is no longer a straight line, but the segment of a circle, described from one point of the surface to another, and whose centre is precisely that of the globe itself. This is accordingly in perfect harmony with the case in question; for to travel from the point M in the equator to the pole B, the arch of the meridian M L B, which I have represented as the shortest road, is in reality a segment of the circle whose centre is precisely that of the earth.

In like manner, if we consider the spot L situated in the meridian B L M A, the shortest road to go thence to the pole B will be the arch L B; and if we know the number of degrees which this arch contains, allowing 69 English miles to a degree, we shall have the length of the road. But if you were disposed to travel from the same spot to the equator by the shortest track, it would be necessary to pursue the track of the arch of the meridian L M, the number of degrees contained in which, reckoning 69 English miles to a degree, would give the distance.

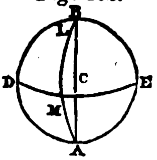
We must be satisfied with expressing these distances in degrees, it being so easy to reduce them to English miles, or the miles of any other nation. Taking, then, the city of *Berlin* for the spot L, we find that the arch L M, which leads to the equator, contains 52 degrees and a half; consequently, to travel from Berlin to the equator, the shortest road is 3623 English miles. But if any one were to go from Berlin to the north pole, he must follow the direction of the arch B L, which, containing 37 degrees and a half, would be 2591 English miles. These two distances added give 6214 English miles for the extent of the arch B L M, which is the fourth part of a circle, or 90 degrees, which contain, as we have seen, 24,855 English miles.

22d August, 1761.

LETTER XLII.

Of Latitude, and its Influence on the Seasons, and the Length of the Day.

Fig. 102.



I BEGIN once more with the same figure (*Fig.* 102), which must by this time be abundantly familiar to you. The whole circle represents the globe of the earth; the points A and B its two poles; B the north or arctic, and A the south or antarctic; so that the straight line B A drawn within the earth and passing through its centre C, is the axis of it. Again, D M E is the equator, which divides it into two hemispheres, D B E the northern, and D A E the southern.

Let us now take any spot whatever, say L, and draw its meridian B L M A, which, being a semicircle, passes through the point L, and the two poles B and A. This then is the meridian of the place L, divided by the equator at M into two equal parts, making two-fourths of a circle, each of which contains 90 degrees. I remark further, that the arch L M of this meridian gives us the distance of the place L from the equator, and that the arch L B expresses the distance of the same place L from the pole B.

This being laid down, it is of importance to observe that the arch L M, or the distance of L from the equator, is denominated *the latitude of the place* L; so that the latitude of any place on the globe is nothing else but the arch of the meridian of that place, which is intercepted between the equator and the given place; in other words, the latitude of a place is the distance of that place from the equator; expressing such distance by degrees, the quantity of which we perfectly know as each degree contains 69 English miles.

You will readily comprehend that this distance must be distinguished according as the given place is in the northern or southern hemisphere. In the former case, that is, if the given place is in the northern hemisphere, we say it has *north latitude;* but if it is in the southern hemisphere, we say it is in *south latitude.*

Taking Berlin as an instance, we say it is in 52 degrees and 32 minutes of north latitude; the latitude of Magdeburg is, in like manner, northern, 52 degrees and 8 minutes. But the latitude of Batavia in the East Indies is 6 degrees 12 minutes south; and that of the Cape of Good Hope, in Africa, is likewise south 33 degrees 55 minutes.

I remark, by-the-way, that for the sake of abbreviation, instead of the word *degree* we affix a small cipher (°) to the numeral characters, and instead of the word *minute* a small slanting bar (′), and instead of *second* two of these (″); thus the latitude of the observatory at Paris is 48° 50′ 14″ N., that is, 48 degrees, 50 minutes, and 14 seconds north. In Peru there is a place named Vlo, whose latitude has been found to be 17° 36′ 15″ S., that is, 17 degrees, 36 minutes, and 15 seconds south. Hence you will understand, that if a place were mentioned whose latitude was 0° 0′ 0″, such a place would be precisely under the equator, as its distance from the equator is 0, or nothing; and in this case it is unnecessary to affix the letter N or S. But were it possible to reach a place whose latitude was 90° N., it would be precisely the north pole of the earth, which is distant from the equator the fourth of a circle, or 90 degrees. This will give you a clear idea of what is meant by the latitude of a place, and why it is expressed by degrees, minutes, and seconds.

It is highly important to know the latitude of every place, not only as essential to geography, in the view of assigning to each its exact situation on geographical charts, but likewise because on the latitude depend the seasons of the year, the inequalities of day and night, and consequently the temperature of the place. As to places situated directly under the equator, there is scarcely any perceptible variation of the seasons; and through the whole year the days and nights are of the same length, namely, 12 hours. For this reason the equator is likewise denominated the equinoctial line; but in proportion as you remove from the equator, the more remarkable is the difference in the seasons of the year, and the more likewise the days exceed the nights in summer; whereas, reciprocally, the days in winter are as much shorter than the nights.

You know that the longest days, in these northern latitudes, are towards the commencement of our summer, about the 21st of June; the nights, of consequence, are then the shortest: and that towards the beginning of our winter, about the 23d of December, the days are shortest and the nights longest: so that everywhere the longest day is equal to the longest night. Now in every place the duration of the longest day depends on the latitude of the place. Here, at Berlin, the longest day is 16 hours and 38 minutes, and consequently the shortest day in winter is 7 hours 22 minutes. In places nearer the equator, or whose latitude is less than that of Berlin, which is 52° 32′, the longest day in summer is less than 16 hours 38 minutes, and in winter the shortest day is more than 7 hours 22 minutes. The contrary of this takes place on removing farther from the equator: at Petersburg, for example, whose latitude is 59° 56′, the longest day is 18 hours 30 minutes, and consequently the night is then only 5 hours 30 minutes: in winter, on the contrary, the longest night is 18 hours 30 minutes, and then the day is only 5 hours 30 minutes. Were you to remove still farther from the equator, till you came to a place whose latitude was 66° 30′, the longest day there would be exactly 24 hours, in other words, the sun would not set at that place at that season; whereas in winter the contrary takes place, the sun not rising at all on the 23d of December, that is, the night then lasting 24 hours. Now at places still more remote from the equator, and consequently nearer the pole, for example, at Warthuys, in Swedish Lapland, this longest day lasts for the space of several days together, during which the sun absolutely never sets; and the longest night, when the sun never rises at all, is of the same duration.

Were it possible to reach the pole itself, we should have day for six months together, and during the other six perpetual night. From this you comprehend of what importance it is to know accurately the latitude of every spot of the globe.

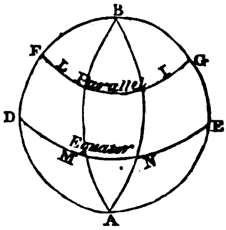
22d August, 1761..

LETTER XLIII.

Of Parallels, of the First Meridian, and of Longitude.

Having informed you, that in order to find the meridian of any given place L, it is necessary to draw on the surface of the earth a semicircle B L M A, passing through the two poles B and A, and through the given place L; I remark, *Fig.* 103, that there is an infinite number of other places through which this same meridian passes, and which are consequently all said to be situated under the same meridian, whether in the northern hemisphere, between B and M, or in the southern, between M and A.

Fig. 103.



Now, all the places situated under the same meridian differ as to latitude, some being nearer to, or more remote from, the equator than others. Thus, the meridian of Berlin passes through the city of Meisse, and nearly through the port of Trieste, as well as many other places of less note.

You will likewise please to observe, that a great many places may have the same latitude, that is, may be equally distant from the equator, but all of them situated under different meridians. In fact, if L is the city of Berlin, whose latitude, or the arch L M, contains 52° 32′, it is possible that there should be under any other meridian B N A, a place I, the latitude of which, or the arch I N, shall likewise be 52° 32′; such are the points F and G, taken in the meridians B D A, B E A. And as a meridian may be drawn through every point of the equator, in which there shall be a place whose latitude is the same with that of Berlin, or the place L, we shall have an infinite number of places all of the same latitude. They will be all situated in the circle F L I G, all the points of which being equally distant from the equator, it is denominated a *parallel circle* to the equator, or simply a *parallel*. A parallel on the globe, then, is nothing else but a circle parallel to the equator, that is, all the points of which are equidistant from it; hence it is evident that all the points of a parallel are likewise equidistant from the poles of the earth.

As it is possible to draw such a parallel through every place on the globe, we can conceive an infinite number of them, all differing in respect of latitude, each having a latitude, whether north or south, peculiar to itself.

You must likewise be abundantly sensible, that the greater the latitude is, or the nearer you approach to either of the poles, the smaller the parallels become; till at last, on coming to the very poles, where the latitude is 90°, the parallel is reduced to a single point. But, on the contrary, as you approach the equator, or the smaller the latitude is, the greater are the parallels; till at last, when the latitude becomes 0, or nothing, the parallel is lost in the equator. It is accordingly by the latitude that we distinguish them; thus, the parallel of 30° is that which passes through every place whose latitude is 30 degrees; but it is necessary to explain yourself, according as you mean north or south latitude.

On consulting an accurate map, you will observe that Hanover is situated under the same parallel with Berlin, the latitude of both being 52° 32′; and that the cities of Brunswick and Amsterdam fall nearly under the same parallel; but that the meridians passing through these places are different. If you know the meridian and the parallel under which any place is situated, you are enabled to ascertain its actual position on the globe. If it were affirmed, for example, that a certain place is situated under the meridian B N A, and the parallel F L G, you would only have to look where the meridian B N A is intersected by the parallel F L G, and the point of intersection I, will give the true position of the given place.

Such are the means employed by geographers to determine the real situation of every place on the face of the globe. You have only to ascertain its parallel, or the latitude, and its corresponding meridian. As to the parallel, it is easy to mark and distinguish it from every other; you have only to indicate the latitude or distance from the equator, according as it is north or south: but how describe a meridian, and distinguish it from every other? They have a perfect resemblance, they are all equal to each other, and no one has a special and distinctive mark. It depends therefore upon ourselves to make choice of a certain meridian, and to fix it, in order to refer all others to that one. If, for example, in *Fig.* 103, (*p.* 146), referred to at the beginning, we were to fix on the meridian B D A, it would be easy to indicate every other meridian, say B M A, by simply ascertaining on the equator the arch D M, contained between the fixed meridian B D A and the one in question B M A, adding only in what direction you proceed from the fixed meridian towards the other, whether from east to west, or west to east.

This fixed meridian, to which every other is referred, is called the *first meridian;* and the choice of this meridian being arbitrary, you will not think it strange that different nations should have made a different choice. The French have fixed on the isle of Ferro, one of the Canaries, for this purpose, and draw their first meridian through it. The Germans and Dutch draw theirs through another of the Canary islands, called Teneriffe. But whether you follow the French or German geographers, it is always necessary carefully to mark on the equator the point through which the first meridian passes; from this point you afterward reckon by degrees the points through which all other meridians pass; and both French and Germans have agreed to reckon from west to east.*

If, therefore, in *Fig*. 103, (*p*. 146), to which I have already referred, the semicircle B D A be the first meridian, and the points of the equator M and N were situated towards the east, you have only, in order to mark any other meridian, say B M A, to indicate the magnitude of the arch D M; and this arch is what we call the *longitude* of all the places situated under the meridian B M A. In like manner, all the places situated under the meridian B N A have their longitude determined by the arch of the equator D N, expressed in degrees, minutes, and seconds.

29*th August*, 1761.

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* In English maps the meridian of Greenwich, a village near London, where the Royal Observatory is situated, is made the first meridian. In American maps the meridian of the city of Washington is generally taken.—*Am. Ed.*

LETTER XLIV.

Choice of the First Meridian.

You have now received complete information respecting what is denominated the latitude and the longitude of a place on the surface of the globe. Latitude is computed on the meridian of the given place, up to the equator; in other words, it is the distance of the parallel passing through that place from the equator; and to prevent all ambiguity, it is necessary to express whether this latitude or distance is north or south.

As to longitude, we must determine the distance of the meridian of the given place from the first meridian; and this distance is computed on the equator, from the first meridian to the meridian of the given place, always proceeding from west to east; in other words, longitude is the distance of the meridian of the given place from the first computing the degrees on the equator, as I have just now said.

We always compute, then, from the first meridian eastward; and it is evident, that when we have computed up to 360 degrees, we are brought back precisely to the first meridian, as 360 degrees complete the circumference of the equator. Accordingly, were any particular place found to be in the 359th degree of longitude, the meridian of that place would be only one degree distant from the first meridian, but towards the west. In like manner, 350° of longitude would exactly correspond with a distance of 10° westward. For this reason, in order to avoid all ambiguity in determining longitude, we go on to reckon up to 360° towards the east.

You will no doubt have the curiosity to know why geographers, in settling the first meridian, have agreed to fix on one of the Canary Islands. I beg leave to reply, that the intention was to begin with settling the limits of Europe towards the west; and as these islands, called the Canaries, and situated in the Atlantic Ocean, beyond Spain, towards America, were still considered as part of Europe, it was thought proper to draw the first meridian through the most remote of the Canary Islands, that we might be enabled to compute the other meridians without interruption, not only all over Europe, but through the whole extent of Asia; from whence, going on to reckon towards the east, we arrive at America, and thence return at length to the first meridian.

But to which of the Canary Isles shall we give the preference? Certain geographers of France made choice of the isle of Ferro, and the Germans that of Teneriffe, because the real situation of these isles was not then sufficiently ascertained, and it was not perhaps known which of them was the most remote; besides, the German geographers imagined that the mountain named the Peak of Teneriffe was pointed out, as it were, by the hand of Nature for the first meridian.

Be this as it may, it seems rather ridiculous to draw the first meridian through a place whose real position on the globe is not perfectly determined; for it was not till very lately that the situation of the Canaries was ascertained. For this reason the most accurate astronomers fix the first meridian precisely 20 degrees distant from that of the observatory at Paris, without regarding through what spot the first may in that case pass; and it is undoubtedly the surest method that can be adopted; and in order to determine every other meridian, the simplest way is to find out its distance from that of Paris: then, if that other meridian is more to the east, you have only to add to it 20 degrees, in order to have the longitude of the places situated under it; but if this meridian be westward to that of Paris, you must

subtract the distance from 20 degrees. Finally, if this distance towards the west is more than 20 degrees, you subtract it from 380 degrees, that is, from 20 degrees above 360, in order to have the longitude of the meridian.

Thus, the meridian of Berlin being to the eastward of the meridian of Paris 11° 2′, the longitude of Berlin will be 31° 2′; and this is likewise the longitude of all other places situated under the same meridian with Berlin.

In like manner, the meridian of Petersburg being 28° more to the east than that of Paris, the longitude of Petersburg will be 48°.

The meridian of St. James's, London, is more to the west than that of Paris by 2° 25′ 15″; subtracting, therefore, that quantity from 20°, the remainder, 17° 34′ 45″, gives the longitude of St. James's, London.

Let us now take the city of Lima in Peru, the meridian of which is 79° 27′ 45″ to the westward of that of Paris; that distance must be subtracted from 380 degrees; which will leave a remainder of 310° 32′ 15″, the longitude of Lima.*

Now, when the latitude and longitude of a place are known, we are enabled to ascertain its true position on the terrestrial globe, or on a map; for as the latitude marks the parallel under which the place is situated, and the meridian gives the meridian of the same place, the point where the parallel intersects the meridian will be exactly the place in question.

You have but to look at a map, that of Europe, for example, and you will see the degrees of the parallels marked on both sides, or their distances from the equator; above and below are the degrees

* This method of reckoning the longitude is now entirely abandoned. The English reckon it from Greenwich, the French from Paris, and so on.—*Ed.*

of longitude, or the distances of the several meridians from the first.

The parallels and meridians are usually traced on maps, degree by degree, sometimes at the distance of five degrees from each other. In most maps the meridians are drawn up and down, and the parallels from left to right: the upper part is directed towards the north, the under to the south, the right-hand side towards the east, and the left-hand side towards the west.

It is likewise to be remarked, that as all the meridians meet at the two poles, the more any two meridians approach to either of the poles the smaller their distance becomes; at the equator their distance always is greatest. Accordingly on all good maps, where the meridians are traced, you will observe that they gradually approximate towards the top, that is, the north; and their distances increase as you proceed towards the equator. This is all that seems to be requisite for the understanding of geographical charts by means of which an attempt is made to represent the surface, or part of the surface, of the globe.

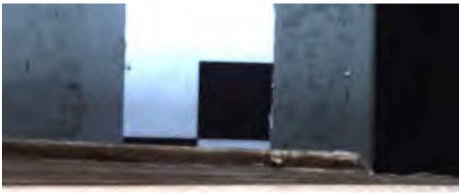
But my principal object was to demonstrate how the real position of every spot on the globe is determined by its latitude and longitude.

1st September, 1761.

LETTER XLV.

Method of determining the Latitude, or the Elevation of the Pole.

It being a matter of such importance to know the latitude and longitude of every place, in order to ascertain exactly the spot of the globe where you are, you must be sensible that it is equally important



to discover the means of certainly arriving at such knowledge.

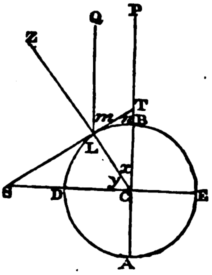
Nothing can be more interesting to a man who has been long at sea, or after a tedious journey through unknown regions, than to be informed at what precise spot he is arrived; whether or not he is near some known country, and what course he ought to pursue in order to reach it. The only means of relieving such a person from his anxiety would undoubtedly be to give him the latitude and longitude of the place where he is; but what must he do to attain this most important information? Let us suppose him on the ocean, or in a vast desert, where there is no one whom he can consult. After having ascertained, by the help of a terrestrial globe, or of maps, the latitude and longitude of the place where he is, he will with ease from them determine his present position, and be furnished with the necessary information respecting his future progress.

I proceed therefore to inform you that it is by astronomy chiefly we are enabled to determine the latitude and longitude of the place where we are; and that I may not tire you by a tedious detail of all the methods which astronomers have employed for this important purpose, I shall satisfy myself with presenting a general idea of them, trusting that this will be sufficient to convey to you the knowledge of the principles on which every method is founded.

I begin with the latitude, which is involved in scarcely any difficulty; whereas the determination of the longitude seems hitherto to have defied all human research, especially at sea, where the utmost precision is requisite. For the discovery of this last, accordingly, very considerable prizes have been proposed, as an encouragement to the learned to direct their talents and their industry towards a discovery so interesting, both from its own importance and from the honour and emolument which are to be the fruit of it.

I return to the latitude, and the means of ascertaining it, referring to some future opportunity a more ample discussion of the longitude, and of the different methods of discovering it, especially at sea.

Fig. 104.



Let the points B and A, *Fig.* 104, be the poles of the earth; B A its axis, and B its centre; let the semicircle B D A represent a meridian, intersected by the equator at the point D; and B D, A D, will be each the quadrant of a circle, or an arch of 90 degrees; the straight line D C will therefore be a radius of the equator, and D E its diameter.

Let there now be assumed in this meridian B D A the point L, the given place of which the latitude is required; or, in other words, the number of degrees contained in the arch L D, which measures the distance of the point L from the equator; or again, drawing the radius C L, as the arch L D measures the angle D C L, which I shall call *y*, this angle *y* will express the latitude of the place L, which we want to find.

Now, it being impossible to place ourselves at the centre of the earth, from which we could take the measure of that angle, we must have recourse to the heavens. There the prolongation of the axis of the earth A B terminates in the north pole of the heavens P, which we are to consider as at an immense distance from the earth. Let the radius C L likewise be carried forward till it terminate in the heavens at the point Z, which is called the zenith of the place; then, drawing through the point L the straight line S T, perpendicular to the radius C L, you will recollect that this line S T is a tangent of the circle, and that consequently it will be horizontal to the place

L; our horizon always touching the surface of the earth at the place where we are.

Let us now look from L towards the pole of the heavens P, which being infinitely distant, the straight line L Q directed to it will be parallel to the line A B P, that is, to the axis of the earth: this pole of the heavens will appear, therefore, between the zenith and the horizon L T; and the angle T L Q, indicated by the letter m, will show how much the straight line L Q, in the direction of the pole, is elevated above the horizon; hence this angle m is denominated the *elevation of the pole.*

You have undoubtedly heard frequent mention made of the elevation of the pole, or, as some call it, the *height of the pole;* which is nothing else but the angle formed by the straight line L Q in the direction of the pole and the horizon of the place where we are. You have a perfect comprehension of the possibility of measuring this angle m, by means of an astronomical instrument, without my going into any further detail.

Having measured this angle m, or the height of the pole, it will give you precisely the latitude of the place L, that is, the angle y. To make this appear, it is only necessary to demonstrate that the two angles m and y are equal.

Now the line L Q being parallel to C P, the angles m and n are alternate, and consequently equal. And the line L T being perpendicular to the radius C L, the angle C L T of the triangle C L T must be a right angle, and the other two angles of that triangle, n and x, must be together equal to a right angle. But the arch B D being the quadrant of a circle, the angle B C D must likewise be a right angle; the two angles x and y, therefore, are together equal to the two angles n and x. Take away the angle x from both, and there will remain the angle y equal to the angle n; but the angle n has been proved equal to the angle *m*, therefore the angle *y* is likewise equal to the angle *m*.

It has already been remarked that the angle *y* expresses the latitude of the place L, and the angle *m* the elevation or height of the pole at the same place L; the latitude of any place, therefore, is always equal to the height of the pole at that same place. The means which astronomy supplies for observing the height of the pole indicate therefore the latitude required.

Astronomical observations made at Berlin have accordingly informed us that there the height of the pole is 52° 32′, and hence we conclude that the latitude of that city is likewise 52° 32′.

This is one very remarkable instance to demonstrate how the heavens may assist us in the attainment of the knowledge of objects which relate only to the earth.

5th September, 1761.

LETTER XLVI.

Knowledge of the Longitude, from a Calculation of the Direction, and of the Space passed through.

I NOW proceed to the longitude; and remark that, on taking a departure, whether by land or water, from a known place, it would be easy to ascertain the spot we had reached, did we know exactly the length of the road, and the direction which we pursued. This might, in such a case, be effected even without the aid of astronomy; and this obliges me to enter into a more particular detail on the subject.

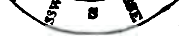
We measure the length of a road by feet; we know how many feet go to a mile, and how many miles go to an arch of one degree upon the globe: thus we are enabled to express in degrees the distance we have travelled.

As to the route or direction in which we travel, it is necessary accurately to know the position of the meridian at every place where we are. As the meridian proceeds in one direction towards the north pole, and in the other towards the south, you have only to draw, on the horizon of the spot where you are, a straight line from north to south, which is called the *meridian line* of that place. All possible care must be taken to trace this meridian line very accurately, and here the heavens must again perform the office of a guide.

You know it is midday when the sun is at his greatest elevation above the horizon; or, which is the same thing, the direction of the sun is then exactly south, and the shadow of a staff fixed perpendicularly on a horizontal plane will fall, at that instant, precisely northward. Hence it is easy to comprehend how an observation of the sun may furnish us with the means of accurately tracing a meridian line, wherever we may be.

Having traced a meridian, every other direction is very easily determined.

Fig. 105.

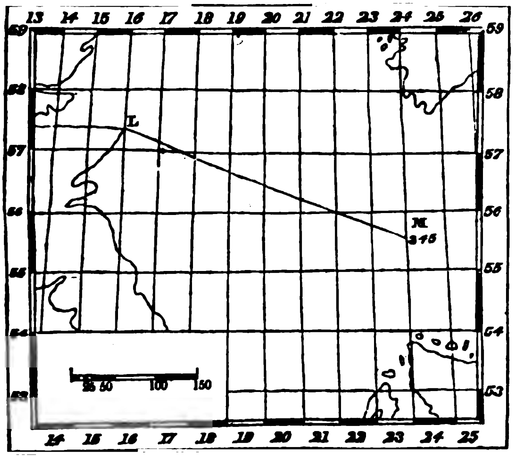


Let the straight line N S, *Fig.* 105, be the meridian, one of the extremities N being directed towards the north, and the other S towards the south. With this meridian let there be drawn at right angles the straight line E W, whose extremity E shall be directed towards the east, and the other extremity W towards the west. Having divided the circle into sixteen equal parts, we shall have so many different directions, denominated according to the letters affixed to them; and in case of not pursuing a direction which exactly corresponds with some one of the sixteen, the angle must be marked which that deviating line of direction makes with the meridian N S, or with E W, which is perpendicular to it.

It is thus we are enabled to determine exactly the direction which we pursue in travelling; and so long as we are assured of the length of the way, and of the direction pursued, it will be very easy to ascertain the true place at which we have arrived, and to indicate both its longitude and latitude. We employ for this purpose an accurate map, which contains the point of departure, and that which we have reached; and by means of the scale, which gives the quantity of miles or leagues that go to a degree, it is easy to trace, on such map, the track pursued and completed.

Fig. 106 represents a map, on which are marked from left to right the degrees of longitude, and those

Fig. 106.



of latitude from top to bottom; it is likewise visible on the face of it, that the meridians converge as they approach towards the north, and retire from each other towards the south, as is the actual case on the globe.

This map contains part of the surface of the earth, from the 53d degree of north latitude to the 59th degree; and from the 13th degree of longitude to the 26th.

Suppose, then, I take my departure from the place L, the longitude of which is 16°, and the latitude 57° 20′, and that I proceed in the direction E S E, and have travelled a space of 345 English miles. In order to determine the longitude and latitude of the place I have reached, I draw from the place L the straight line L M, making with the meridian an angle of 67° 30′, the same angle which the direction E S E in the preceding figure makes with N S. Then on that line I take, according to the scale marked on the chart, L M equal to 345 English miles, and the point M shall be the place which I have reached.

I have then only to compare this place with the meridians and parallels traced on the map, and I find that its longitude is 24° nearly; and on measuring more exactly the part of the degree to be added to the 24th degree, I find the longitude of the point M to be 24° 4′. As to the latitude, I observe it to be between the 55th and 56th degree, and by an easy computation I find it to be 55° 25′; so that the latitude of the place M, which I have reached, is 55° 25′, and its longitude 24° 4′.

It has here been supposed that I have invariably pursued the same direction, E S E, from first to last; but if I have from time to time deviated from that direction, I have only to perform the same operation on each deviation, to find the place where I then was; from this I take a fresh departure, and trace my direction till another deviation takes place; and so on, till I reach my object. By these means it is always in my power, whether travelling by sea or land, to ascertain the place I have reached; provided I know exactly, through my whole progress, the direction I pursue, and measure with equal accuracy the length of the way.

We might in this case dispense even with the assistance of astronomy, unless we had occasion for it accurately to determine our direction, or the angle which it makes with the meridian; but the magnetic needle or compass may, in many cases, supply this want.

You must be sensible, however, that it is possible to make a very considerable mistake, both in the computation of the direction and of the length of the way, especially in very long voyages. How often is it necessary to change the direction in travelling even from hence to Magdeburg? and how is it possible to measure exactly the length of the way? But when we travel by land we are not reduced to this expedient; for we are enabled to measure by geometrical experiments the distance of places, and the angles which the distances make with the meridian of every place; and thus we can determine, with tolerable accuracy, the true situation of all places.

8th September, 1761.

LETTER XLVII.

Continuation. Defects of this Method.

A METHOD of observing the direction pursued and the length of the course, seems to be of singular utility in sea voyages, because there we are not under the necessity of deviating from the direction every moment, as in travelling by land; for with the same wind we can proceed in the same direction.

Pilots are accordingly very attentive in exactly observing the course of the vessel, and in measuring the progress she has made. They keep an accurate

journal of all these observations at the close of every day, nay still more frequently; they trace on their sea-charts the progress they have made, and thus are enabled to mark on the charts, for every period of time, the point where they are, and of which they consequently know the latitude and longitude. Accordingly, so long as the course is regular, and the vessel is not agitated by a tempest, good pilots are seldom mistaken; but when they are in doubt, they have recourse to astronomical observations, from which they discover the elevation of the pole; and this being always equal to the latitude of the place where they are, they compare it with that which they have marked on the chart, conformably to the computation of their progress. If these are found to coincide, their computation is just; if they discover a difference, they conclude with certainty that some error has been committed in the computation of the distance and of the course; in that case they re-examine both the one and the other more carefully, and endeavour to apply the necessary corrections, in order to make the computation agree with the observation of the height of the pole, or of the latitude, which is equal to it.

This precaution may be sufficient in short voyages, as the errors committed can in these be of no great importance; but in very long voyages, these slight mistakes may accumulate to such a degree that at last a very gross mistake may be committed, and the place where the vessel actually is may differ considerably from what it was supposed to be on the chart.

I have hitherto gone on the supposition that the voyage proceeded quietly; but should a storm arise, during which the vessel is subjected to the rudest concussions of wind and waves, it is evident that the computation of distance and course is entirely deranged, and that it is impossible to trace on the chart the progress she has made.

It would be very easy after this derangement to ascertain by astronomical observations the latitude of the ship's place; but this would determine only the parallel of that place, and it would remain totally uncertain at what point of the parallel she actually was.

It is necessary therefore to discover likewise the longitude of the place, which shows us the meridian under which it is situated; and then the intersection of that meridian with the parallel found will give the vessel's true place. This will make you sensible of what importance it is to assist mariners in discovering likewise the longitude of the place where they are.

This necessity is imposed not only from the consideration of the tempests to which navigation is liable; for it is possible, supposing the voyage to proceed ever so quietly, to be grossly mistaken in the computation of both course and distance. Could we suppose the sea to be at rest, it might be possible to invent various methods of ascertaining with tolerable exactness the way which the vessel has made; but there are rapid currents in many places of the ocean, which have the resemblance of a river running in a certain direction. Thus it is observed that the Atlantic Ocean has a perpetual current into the Mediterranean Sea, through the Straits of Gibraltar; and that the ocean between Africa and America has a very considerable current from east to west, so that a voyage to America is performed in much less time than a voyage from America to Europe.

Were such currents constant and well known, we should have considerable assistance towards forming our calculations; but it has been observed that they are sometimes more, sometimes less rapid, and that they frequently change their direction; which deranges the calculations of the most skilful navigator to such a degree that it is no longer safe to

trust them. We have but too many fatal instances of ships dashed on concealed rocks and lost, because these were computed to be still at a considerable distance. It was afterward discovered, when too late, that these calamities had been occasioned by the currents of the ocean, which deranged the calculations of navigators.

In fact, when the ocean has a current which makes it flow like a river, following a certain direction, vessels caught in it are carried away imperceptibly. In a river we clearly perceive that the current is carrying us along, by observing the banks or the bottom; but at sea no land is visible, and the depth is too great to admit of our making any observation from the bottom. At sea, then, it is impossible to discern the currents; and hence so many dreadful mistakes respecting both course and distance. Whether, therefore, we take tempests into the account or not, we are always under the necessity of falling on other methods of ascertaining the longitude of the places where we may arrive; and of the various methods hitherto employed for acquiring this knowledge of the longitude I now proceed to inform you.

12*th September*, 1761.

LETTER XLVIII.

Second Method of determining the Longitude, by means of an exact Timepiece.

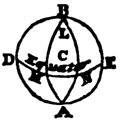
A VERY sure method of finding the longitude would be a clock, watch, or pendulum, so perfect, that is to say, which should always go so equally and so exactly, that no concussion should be able to affect its motion.

Supposing such a timepiece constructed, let us see in what manner, by means of it, we should be enabled to solve the problem of the longitude. We must return, for this purpose, to the consideration of meridians, which we are to conceive to be drawn through every place on the surface of the globe.

You know that the sun seems to describe every day a circle round the earth, and that, of consequence, he passes successively over all the meridians in the space of twenty-four hours.

Now, the sun is said to pass *over* or *through* a given meridian, if a straight line drawn from the sun to the centre of the earth C, *Fig.* 107, pass precisely through that meridian. If, therefore, in the present case the line drawn from the sun to the centre of the earth pass through the meridian B L M A, we would say that the sun was in that meridian, and then it would be midday to all the places situated under this meridian; but under every other it would not be midday at that precise instant; it would there be before noon or after it everywhere else.

Fig. 107.



If the meridian B N A is situated to the eastward of the meridian B M A, the sun, in making his circuit from east to west, must pass over the meridian B N A before he reaches the meridian B M A; consequently it will be midday under the meridian B N A earlier than under the meridian B M A; when, therefore, it shall be midday under this last meridian, midday under every other meridian to the eastward will be already past, or it will be afternoon with them. On the contrary, it will be still forenoon under every meridian, say B D A, situated to the westward, as the sun cannot reach it till he has passed over the meridian B M A.

And as the motion of the sun is regular and uniform, and he completes his circuit of the globe, that is 360 degrees, in twenty-four hours, he must every hour describe an arch of 15 degrees. When, therefore, it is noon at Berlin, and at every other place situated under the same meridian, noon will be

already past under meridians situated to the eastward; and more particularly still under the meridian situated 15 degrees to the eastward of that of Berlin, it will already be one o'clock; under the meridian 30 degrees eastward, two o'clock; under that of 45 degrees, three o'clock afternoon, and so on. The contrary will take place under meridians situated to the westward of that of Berlin; when it is noon there, it will be only eleven o'clock forenoon under the meridian 15 degrees to the westward, ten o'clock under the meridian of 30, nine o'clock under the meridian of 45 degrees westward, and so on; a difference of 15 degrees between two meridians always amounting to an hour of time.

To elucidate still more clearly what has now been remarked, let us compare the two cities Berlin and Paris. As the meridian of Berlin is 11° 17′ 15″ to the eastward of that of Paris, reckoning an hour to 15 degrees, this difference of 11° 17′ 15″ will give 44 minutes and 29 seconds of time, or three-quarters of an hour nearly. When, therefore, it is midday at Paris, it will be 44 minutes and 29 seconds after midday at Berlin; and reciprocally, when it is midday at Berlin, it will only be 15 minutes and 31 seconds after eleven o'clock at Paris; so that it will not be noon at this last city till 44 minutes and 29 seconds afterward. Hence it is evident, that the clocks at Berlin should always be faster than those of Paris, and that this difference ought to be nearly 44 minutes and 29 seconds.

The difference between the meridians of Berlin and Magdeburg is nearly 1° 40′; Berlin therefore is to the eastward of Magdeburg; and this difference reduced to time gives 6 minutes and 40 seconds, which the clocks of Berlin ought to indicate more than that of Magdeburg. Consequently, if it is just now noon at Magdeburg, and the clocks there, which I suppose well regulated, point to XII., the clocks at Berlin should, at the same instant, indicate 6 minutes and 40 seconds after XII., that is, noon there is already past.

Hence you see, that in proportion as places differ in longitude, or as they are situated under different meridians, well-regulated timepieces ought not to point out the same hour at the same instant, but the difference ought to be a whole hour when that of the longitude is 15 degrees.

In employing a timepiece, then, for ascertaining the longitude of the places through which we pass, it would first be necessary to regulate it exactly at some place where we actually were. This is done by observing the instant of noon, that is, the instant when the sun passes over the meridian of that place; and the timepiece ought then to point precisely to XII. It ought afterward to be adjusted in such a manner, that always after a revolution of 24 hours, when the sun returns to the meridian, the index after having made two complete circuits, should again point exactly to XII. If this is carefully observed, such well regulated timepieces will not coincide in different places, unless these be situated under one and the same meridian; but if they are situated under different meridians, that is, if there be a difference of longitude, the time indicated by the clock or watch, at the same moment, will likewise be different; at the rate of one whole hour of time for every 15 degrees of longitude.

Knowing, then, the difference of time indicated by well regulated timepieces, at different places, and at the same instant, we are enabled exactly to compute the difference of longitude at these two places, reckoning always 15 degrees for an hour, and the fourth part of a degree for a minute.

15th September, 1761.

LETTER XLIX.

Continuation, and further Elucidations.

You will be less surprised at the difference of time which well regulated timepieces must indicate under different meridians, when you recollect, that while it is noon with us, there are countries towards the east where the sun is already set, and that there are others towards the west where he is but just rising. It must therefore be already night with the one, and still morning with the other, at the same instant that it is noon with us. You know, besides, that with our antipodes, who are under the meridian diametrically opposite to ours, it is night, while it is day with us; so that our noon corresponds exactly to their midnight.

It will be an easy matter, after these elucidations, to show how an exact timepiece may assist us in discovering the difference of meridians, or that of the longitude, at different places.

Supposing me possessed of such an excellent timepiece, which, once exactly regulated, shows me every day the precise time it is at Berlin, so that whenever it is noon at Berlin, it points precisely to XII.: supposing further, that it goes so regularly, that once adjusted, I have no further occasion to touch it, and that its motion is not to be deranged either by the shaking of a carriage, or the agitation of a vessel on the ocean, or by any concussion whatever to which it may be exposed.

Provided thus with a timepiece of this description, I set out to travel, whether by land or by sea; perfectly assured, that go where I will, its motion will be steady and uniform, as if I had remained at Berlin: it will every day point to XII. at the very moment it is noon at Berlin, and that wherever I may happen to be. On this journey, I arrive first at Magdeburg: there I observe the sun when he passes the meridian, and this happens when he is exactly south; and it being then noon at Magdeburg, I consult my timepiece, and observe it points to 6 minutes and 40 seconds after XII.: whence I conclude, that when it is noon at Magdeburg, noon at Berlin is already past, and that the difference is 6' 40" of time, which correspond to 1° 40' of distance; therefore the meridian of Magdeburg is to the westward of that of Berlin. The longitude of Berlin, therefore, being nearly 31° 7' 15', the longitude of Magdeburg will be 1° 40' less, that is, it will be 29° 27' 15".

I thence proceed to Hamburgh, accompanied by my timepiece, which I never touch; and there observing when it is noon by the sun, for I cannot depend on the public clocks which there announce the hour, I find my timepiece already announces 13' 33" after XII.; so that at Berlin noon is past 13' 33" when it is exactly noon at Hamburgh: hence I conclude, that the meridian of Hamburgh is 3° 23' 15" to the westward of that of Berlin; reckoning 15° to an hour, that is one degree for every four minutes of time: accordingly, I find that 13' 33" of time give 3° 23' 15" of distance for the difference of the meridians. The longitude of Hamburgh will be of course 27° 44'.

At Hamburgh I go to sea, still accompanied by my timepiece, and after a long voyage I arrive at a place where, waiting for noon, the moment of which I ascertain by observing the sun, I find that my timepiece indicates only 58' 15" after X.; so that then it is not yet noon at Berlin, and the difference of time is 1 hour 1 minute and 45 seconds, from which I conclude, that the place at which I have arrived is to the eastward of Berlin; and as one hour gives 15 degrees, one minute of time 15', and 45 seconds of time 11' 15", the difference of the meridians will therefore be 15° 26' 15".

I find, then, that I am at a place to the eastward of Berlin, whose longitude is greater than that of Berlin by 15° 26′ 15″; now the longitude of that city being nearly 31° 7′ 15″, the longitude of the place where I am must be 46° 33′ 30″. Thus I have discovered under what meridian I now am; but I am still uncertain as to the point of the meridian. In order to ascertain this, I have recourse to astronomical observations, and find the height of the pole to be precisely 41°. Knowing likewise that I am still in the northern hemisphere, as I have not passed the equator, I discover that I actually am at a place whose latitude is 41° north, and longitude 46° 33′ 30″. I take therefore my globe or maps, and trace the meridian whose longitude is 46° 33′ 30″; I look for the place whose latitude is 41°, and at the point of intersection I find I have got to the city of Constantinople without having occasion to apply for information to any person whatever.

Thus, at whatever place of the globe I may arrive, possessed of a timepiece so exact, I am able to ascertain the longitude of it; and then an observation of the height of the pole will show me its latitude. All that remains, therefore, is to take the terrestrial globe, or a good map, and it will be easy for me to ascertain where I am, however unknown to me the country may in other respects be.

It is much to be regretted, that artists of the greatest ability have hitherto been unsuccessful in the construction of timepieces such as I have described, and such as the case requires. We meet with a great many very good pendulum machines, but they go regularly only when fixed in undisturbed situations; the slightest concussion is apt to derange their motion; they are therefore totally useless in long sea voyages. It is obvious that the pendulum, which regulates the motion, is incapable of resisting the shocks to which it is exposed in navigation. About ten years ago, however, an English artist pretended that he had constructed a timepiece proof against the motion of a ship at sea, and that after having tried it a long time together in a carriage on the road, it was impossible to perceive the slightest derangement; on which the inventor claimed and received part of the parliamentary reward proposed for the discovery of the longitude, and the rest was to be paid after it had been put to the proof of a long voyage. But since that time we have heard no more of it; from which it is to be presumed that this attempt too has failed, like many others which had the same object in view.[*]

19th September, 1761.

LETTER L.

Eclipses of the Moon, a third Method of finding the Longitude.

FROM want of the exquisite timepiece of which I have endeavoured to give you an idea, the eclipses of the moon have hitherto been considered as the most certain method of discovering the longitude; but these phenomena present themselves so rarely, that we have it not in our power to employ them so often as occasion requires.

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[*] The attempt has by no means failed. The reward offered by the French Academy, and more especially the liberal reward offered by the British parliament of 20,000*l*., for a discovery that should determine the longitude within half a degree, provided such method should extend more than 80 miles from the coast, stimulated the ingenuity of various mechanicians, and led to the discovery of several means by which the expansion and contraction of metals by heat and cold (the principal cause of irregularity in the best timekeepers) were very nearly compensated, and an equable motion established. The large reward of 20,000*l*. was gained by Harrison, for his invention of the gridiron pendulum, and the application of the same principle to a watch to effect a self-regulating curb for limiting the effective length of the spiral pendulum spring. This reward, which it is said was actually increased by gratuities of the Board of Longitude, the East India Company, and others, to 24,000*l*., was paid to James and William Harrison, father and son, in 1774, and a new act of parliament was passed, granting further but less rewards for still greater perfection in the construction of chronometers. By the successive labours of Mudge, Arnold, Earnshaw, and others, the art of chronometer making has been brought to so great perfection, as to render this instrument of the highest value to the navigator, and to bring it within the reach of almost every ship-master.—*Am. Ed*

You know that the moon is eclipsed when it passes into the shadow of the earth: it is possible then to observe the moment when the moon begins to enter into the shade, and when she has emerged; the one is denominated the beginning of the eclipse, and the other its end; and when both are observed, the mean time between them is denominated the middle of the eclipse. The moon is sometimes wholly immerged in the shadow of the earth, and remains for some time invisible; this we call a total eclipse, during which we may remark the moment when the moon entirely disappears, and that when she begins to emerge; the former is called the beginning of total darkness, and the latter the end of it. But when a part only of the moon is obscured, we call it a partial eclipse; and we can remark only the moment of its beginning and ending. You know likewise that eclipses of the moon can happen only at the full, and that but rarely.

When, therefore, an eclipse of the moon is observed at two different places situated under different meridians, the beginning of the eclipse will be clearly seen at both, and at the same instant; but the timepieces at these different places will by no means indicate the same hour, or any other division of time exactly the same: I mean well regulated timepieces, each of which points precisely to XII. when it is noon at that place. If these places are situated under the same meridian, their timepieces will no doubt indicate the same time at the beginning and at the end of the eclipse. But if these two meridians are 15 degrees distant from each other, that is, if the difference of their longitude be 15°, the timepieces must differ a complete hour from the beginning to the end of the eclipse; the timepiece of the place situated to the eastward will indicate one hour more than the other: the difference of 30° in longitude will occasion that of two hours in the time indicated by well regulated clocks or watches; and so on, according to the following table:

DIFFERENCE OF LONGITUDE.		DIFFERENCE OF LONGITUDE.	
Of Degrees.	Of Time.	Of Degrees.	Of Time.
15°	1 Hour.	105	7 Hours.
30	2	120	8
45	3	135	9
60	4	150	10
75	5	165	11
90	6	180	12

If, therefore, the difference of the longitude were 150°, the timepieces would differ ten hours from the beginning to the end of the eclipse.

Thus, when the same eclipse is observed at two different places, and the moment of its commencement is exactly marked on the timepieces at each, it will be easy to calculate from the difference of the time indicated, the difference of longitude between the two places. Now, that where the time is more advanced must be situated more towards the east, and consequently its longitude greater, as longitude is reckoned from west to east.

By such means, accordingly, the longitude of the principal places on the globe have been determined, and geographical charts are constructed conformably to these determinations. But it is always necessary to compare the observations made in a place the longitude of which was not already known, with those which had been made in a known place, and to wait the result of that comparison. Were I to arrive, then, after a long voyage, at an unknown place, and an opportunity presented itself of there observing an eclipse of the moon, this would, in the first instance, afford me no assistance towards the discovery of the longitude of that place; I could not, till after my return, compare my observation with another made in a known place, and thus I should learn too late where I was at that time. The grand point in request is, How am I at the moment to acquire the necessary information, that I may take my measures accordingly?

Now, the motion of the moon being so exactly known, it is possible to attain this satisfaction; for we are thereby enabled, not only to calculate beforehand all future eclipses, but to ascertain the moment of the beginning and end, according to the timepieces of a given place. You know that our Berlin almanacs always indicate the beginning and the end of every eclipse visible at that city. In the view, then, of undertaking a long voyage, I can furnish myself with a Berlin almanac; and if an opportunity presents itself of observing an eclipse of the moon at an unknown place, I must mark exactly the time of it by a timepiece accurately regulated by the sun at noon, and compare the moments of the beginning and end of the eclipse with those indicated in the almanac, in order to ascertain the difference between the meridian of Berlin and that which passes through the place where I am.

But besides the rarity of eclipses of the moon, this method is subject to a further inconvenience; we are not always able to distinguish with sufficient accuracy the moment of the beginning and end of the eclipse, which comes on so imperceptibly that a mistake of several seconds may very easily be committed. But as the mistake will be nearly the same at the end as at the beginning, we calculate the middle point of time between the two moments observed, which will be that of the eclipse; and we afterward compare this with that which is indicated by the almanac for Berlin, or for any other known place.

If the almanac for next year should not be published when I set out on my voyage, or supposing it to last more years than one, there are books containing the eclipses calculated for several years to come.

22*d September*, 1761.

LETTER LI.

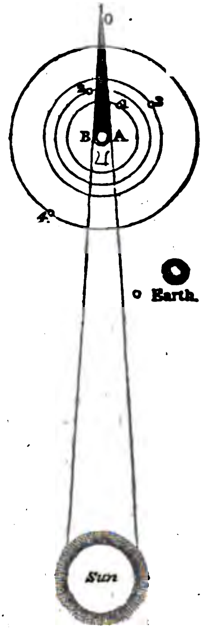
Observation of the Eclipses of the Satellites of Jupiter, a fourth Method of finding the Longitude.

Eclipses of the sun may likewise assist in ascertaining the longitude, but in a way that requires more profound research, because the sun is not immediately obscured; it is only the interposition of the body of the moon which obstructs the transmission of his rays to us, as when we employ a parasol to shelter us from them, which does not prevent others from beholding all their lustre. For the moon conceals the sun only from part of the inhabitants of the earth; and an eclipse of the sun may be clearly visible at Berlin, while at Paris there is no interception of his light.

But the moon is really eclipsed by the shadow of the earth; her own light is diminished or extinguished by it: hence the eclipses of the moon are seen in the same manner wherever she is above the horizon at the time of the eclipse.

It cannot have escaped your penetration, that if there were other heavenly bodies which from time to time underwent any real obscuration, they might be employed with similar success as the eclipses of the moon in ascertaining the longitude. The satellites of Jupiter, which pass so frequently into the shadow of their planet that almost every night one or other of them is eclipsed, may be ranked in the number of these, and furnish us with another excellent method of determining the longitude. Astronomers accordingly employ it with great success.

Fig. 108.



You know that Jupiter has four satellites which make their revolutions round him, each in his own orbit, as represented in the annexed figure, *Fig.* 108, by circles described round Jupiter. I have likewise represented the sun in this figure, in order to exhibit the shadow A O B behind the body of Jupiter. You see the first of these satellites, marked 1, on the point of entering into the shadow; the second, marked 2, has just left it; the third, 3, is still at a great distance, but approaching to it; and the fourth, 4, has left it a considerable time ago.

As soon as one of these satellites passes into the shadow it becomes invisible, and that suddenly; so that at whatever place of the globe you may happen to be, the satellite which was before distinctly visible disappears in an instant. This entrance of a satellite into the shadow of Jupiter is denominated *immersion*, and its departure from the shadow *emersion;* when the satellite which had for some time been invisible suddenly reappears.

The immersions and emersions are equally adapted to the determination of the longitude, as they take place at a decided instant; so that when such a phenomenon is observed at several places of the globe, you must find in the time indicated by the timepieces of each the difference which exactly corresponds to the difference of the distance of their meridians. It is the same thing as if we observed the beginning or the end of an eclipse of the moon; and the case is then involved in no difficulty. For some time past we have been able to calculate these eclipses of the satellites of Jupiter, that is, their immersions and emersions; and we have only to compare the time observed with the time calculated for a given place, say Berlin, in order to conclude at once the distance of its meridian from that of our capital.

This method is accordingly practised universally in travelling by land; but the means have not yet been discovered of profiting by it at sea, where, however, it is of still greater importance for a man to know with certainty where he is. Were the satellites of Jupiter as visible to the naked eye as the moon is, this method would be attended with no difficulty, even at sea; but the observation cannot be made without a telescope of at least four or five feet in length—a circumstance which presents an insurmountable obstacle.

You well know that it requires some address to manage, even on land, a telescope of any length, to direct it towards the object which you wish to contemplate, and to keep it so steady as not to lose the object; you will easily comprehend, then, that a ship at sea being in a continual agitation, it must be almost impossible to catch Jupiter himself; and if you could find him, you would lose him again in a moment. Now, in order to make an accurate observation of the immersion or emersion of one of the satellites of Jupiter, it is absolutely necessary that you should have it in your power to look at him steadily for some time together; and this being impossible at sea, we are to all appearance constrained to abandon this method of determining the longitude.

This inconvenience, however, may be remedied two ways; the one by the construction of telescopes six inches long, or less still, capable of discovering clearly the satellites of Jupiter; and there can be no doubt that these would be more manageable than such as are four or five feet in length. Artists are actually employing themselves with success in bringing telescopes of this sort to perfection; but it has not yet been proved whether or not it will require as much address to point them to the object as those which are longer.

The other way would be to contrive a chair to be used on shipboard, which should remain fixed and motionless, so as not to be affected by the agitation of the vessel. It does not seem impossible that a dexterous mode of balancing might effect this. In fact, it is not long since we read in the public prints that an Englishman pretended that he had constructed such a chair, and therefore claimed the prize proposed for the discovery of the longitude.* His claim was well founded, if he indeed constructed the machine, as it would be possible by means of it to observe at sea the immersions and emersions of the satellites of Jupiter, which are undoubtedly very much adapted to the making of this discovery; but for some time past no further mention has been made of it. From the whole, you must have perceived how many difficulties attach themselves to the discovery of the longitude.

26th September, 1761.

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* The invention here alluded to was Irwin's marine chair, which was tried at sea, but it was not found to answer the purpose of the inventor.—*Ed.*

LETTER LII.

The Motion of the Moon, a fifth Method.

The heavens furnish us with one resource more for discovering the longitude without the assistance of telescopes, in which astronomers seem to place the greatest confidence. It is the moon, not only when eclipsed but at all times, provided she be visible; an unspeakable advantage considering that eclipses are so rare, and that the immersions and emersions of the satellites of Jupiter are of such difficult observation; there being a considerable time every year during which the planet Jupiter is not visible to us, whereas the moon is almost constantly in view.

You must undoubtedly have already remarked, that the moon rises every day almost three-quarters of an hour later than the preceding, not being attached to one fixed place relatively to the stars, which always preserve the same situation with respect to each other, though they have the appearance of being carried round by the heavens, to accomplish every day their revolution about the earth. I speak here according to appearances; for it is the earth which revolves every day round its axis, while the heavens and the fixed stars remain at rest; while the sun and planets are continually changing their place relatively to these. The moon has likewise a motion abundantly rapid from one day to another, with relation to the fixed stars.

If you were to see the moon to-day near a certain fixed star, it will appear to-morrow at the same hour at a considerable distance from it towards the east; and the distance sometimes exceeds even 15 degrees. The velocity of her motion is not always the same, yet we are able to determine it very exactly for every day; by which means we can calculate before-hand her true place in the heavens for every hour of the day, and for any known meridian, say that of Berlin, or Paris.

Suppose, then, that after a long voyage I find myself at sea, in a place altogether unknown, what use can I make of the moon, in order to discover the longitude of the place where I am? There is no difficulty with respect to the latitude, even at sea, where there are means abundantly certain for ascertaining the height of the pole, to which the latitude is always equal. My whole attention, then, will be directed to the moon; I will compare her with the fixed stars which are nearest, and thence calculate her true place relatively to them. You know there are celestial globes on which all the fixed stars are arranged, and that celestial charts are likewise constructed similar to geographical maps, on which are represented the fixed stars which appear in a certain quarter of the heavens. On taking, then, a celestial chart on which the fixed stars to which the moon is near are marked, it will be an easy matter to determine the true place where the moon at that time is; and my watch, which I have taken care to regulate there, from an observation of the moment of noon, will indicate to me the time of my lunar observation. Then, from my knowledge of the moon's motion, I calculate for Berlin, at what hour she must appear in the same place where I have seen her. If the time observed exactly correspond with the time of Berlin, it will be a demonstration that the place where I am is precisely under the meridian of Berlin, and that consequently the longitude is the same. But if the time of my observation is not that of Berlin, the difference will give that which is between the meridians; and reckoning 15 degrees for every hour of time, I compute how much the longitude of the place I am at is greater or less than that of Berlin: the place where time is more advanced has always the greater longitude.

This is an abstract of the manner of determining longitude by simple observations of the moon. I remark, that the happiest moments for successfully performing this operation, and for accurately determining the moon's place, are, when a fixed star happens to be concealed behind her body; this is called *occultation*, and there are two instances favourable to observation, that when the moon in her motion completely covers the star, and that when the star reappears. Astronomers are particularly attentive to catch these instants of occultation, in order to calculate from them the moon's true place.

I foresee, however, an objection you will probably make respecting the time-piece with which I suppose our navigator provided, after having maintained the impossibility of constructing one that shall be proof against every agitation of a ship at sea. But this impossibility respects only such time-pieces as are expected to preserve a regular motion for a long time together, without the necessity of frequent adjustment; for as to the observations in question, a common watch is quite sufficient, provided it go regularly for some hours, after having been carefully adjusted to the noon of the place where we are; supposing a doubt to arise, whether we could calculate from it the succeeding evening or night, at the time we observe the moon, the stars likewise will afford the means of a new and accurate adjustment. For as the situation of the sun with relation to the fixed stars is perfectly known for any time whatever, the simple observation of any one star is sufficient to determine the place where the sun must then be; from which we are enabled to calculate the hour that a well regulated timepiece ought to indicate. Thus, at the very instant of making an observation by the moon, we are enabled likewise to regulate our timepiece

by the stars; and every timepiece is supposed to go regularly for so short a space.

29th September, 1761.

LETTER LIII.

Advantages of this last Method; its Degree of Precision.

This last method of finding the longitude, founded on lunar observations, seems to merit the preference, as the others are subjected to too many difficulties, or the opportunities of employing them occur too seldom to be useful. And you must be abundantly sensible that success depends entirely on the degree of precision attained in forming the calculation, and that the errors which may be committed would lead to conclusions on which we could place no dependence. It is of importance, therefore, to explain what degree of precision we may reasonably hope to attain in reducing this method to practice, founded on the considerable change which the moon undergoes from one day to another in her position. It may be affirmed, that if the moon's motion were more rapid, it would be more adapted to the discovery of the longitude, and would procure for us a higher degree of precision. But if, on the contrary, it were much slower, so that we could scarcely discern any change of her position from day to day, we could derive very little, if any, assistance from her towards the discovery of the longitude.

Let us suppose, then, that the moon changes her place among the fixed stars a space of 12 degrees in twenty-four hours; she will, in that case, change it one degree in two hours, and half a degree, or thirty minutes in an hour: if we were to commit a mistake in observing the moon's place of thirty minutes, it would be the same thing as if we observed the moon an hour earlier or later, and we should commit a mistake of one hour in the conclusion respecting the difference of the meridians. Now, one hour's difference in the meridians corresponds to 15 degrees in their longitude; consequently, we should be mistaken 15 degrees in the longitude itself of the place we look for; which would undoubtedly be an error so enormous that it were almost as well to know nothing about it; and a simple computation of the distance and the direction, however uncertain, could not possibly lead to a mistake so very gross. But a man must have gone to work in a very slovenly manner to commit a mistake of 30 minutes respecting the moon's place; and the instruments which he employed must have been very bad, a thing not to be supposed.

Nevertheless, however excellent the instruments may be, and whatever degree of attention may have been bestowed, it is impossible to keep clear of all error; and he must have acquitted himself very well indeed who has not committed the mistake of one minute in determining the moon's place. Now, as it changes half a degree, or 30 minutes, in one hour, it will change one minute of distance in two minutes of time. When, therefore, the mistake of the moon's place amounts to no more than one minute, the mistake in the difference of meridians will amount to two minutes of time. And one hour, or 60 minutes, being equivalent to 15 degrees of longitude, there will result from it an error of half a degree in the longitude; and this point of precision might be sufficient for every purpose, were it but attainable.

I have hitherto supposed our knowledge of the moon's motion to be so perfect, that, for a known meridian, we could determine the moon's true place for every moment without an error; but we are still very far short of that point of perfection. Within these twenty years, the error in this calculation was more than six minutes; and it is but lately that the ingenious *Professor Mayer* of Gottingen, pursuing the track I had pointed out to him, has succeeded so far as to reduce this error to less than a minute. It may very easily happen, then, that in the calculation likewise, the error of one minute may be committed, which, added to that of a minute committed in the observation of the moon's place, will double that which results from it respecting the longitude of the place where we are; and, consequently, it may possibly amount to a whole degree: it is proper further to remark, that if the moon in twenty-four hours should change her relative situation more than 12 degrees, the error in the longitude would be less considerable. The means may perhaps be discovered of diminishing still further the errors into which we are liable to fall, in the observation and in the calculation; and then we should be able to ascertain the longitude to a degree, or less. Nay, we ought not to despair of attaining a still higher degree of precision. We have only to make several observations, which can be easily done by remaining several days together at the same place. It is not to be apprehended, in that case, that all the conclusions should be equally defective; some will give the longitude sought too great, others too small, and by striking a medium between all the results, we may rest assured that this longitude will not be one degree removed from the truth.

The English nation, generously disposed to engage genius and ability in this important research, has proposed three prizes for ascertaining the longitude —one of 10,000*l*., one of 15,000*l*., and one of 20,000*l*. The first of these is to be bestowed on the person who shall determine the longitude to a degree, or about it, so as to give perfect assurance that the error shall not exceed one degree at most. The second is to be given to him who shall discover a method still more exact, so that the error shall never exceed two-thirds of a degree, or 40 minutes. The highest prize is destined to the man who shall ascertain the longitude so exactly that the error shall never exceed half a degree, or 30 minutes; and a higher degree of precision is hardly to be expected. No one of these prizes has hitherto been allotted: I do not take into the account the gratification bestowed on the artist who pretended to it from his construction of perfect timepieces. *Mr. Mayer* is at this moment claiming the highest, and I think he is entitled to it.*

3d October, 1761.

LETTER LIV.

On the Mariner's Compass, and the Properties of the Magnetic Needle.

You are by this time sufficiently informed respecting the discovery of the longitude: I have had the pleasure of explaining the various methods which have been employed for the determination of it.

The first and most natural is carefully to observe the quantity of space which we have gone over, and the direction in which we moved; but the currents and tempests to which sea voyages are exposed render this method impracticable.

The second requires the construction of a timepiece so perfect as to go always uniformly, notwithstanding the agitation of a ship at sea; which no artist has hitherto been able to accomplish.

The third is founded on the observation of the eclipses of the moon, which would completely

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* The widow of *Professor Mayer* received from the British parliament a reward of 3000*l*. sterling; and *Euler* himself received 300*l*. for furnishing the theorems on which *Mayer's* Tables are founded. The latter received also a reward from the French government, and gained several prizes for his improvement of the lunar theory.—*Ed.*

answer every purpose, were not opportunities of employing it too rare, and least in our power when the necessity may be most urgent.

The fourth refers to the eclipses of the satellites of Jupiter, which would answer the purpose extremely well, had we the means of employing at sea telescopes of a certain description, without which they are invisible.

Finally, observations of the moon herself furnish a fifth method, which appears the most practicable, provided we were able to observe the moon's place in the heavens so exactly, that the error in calculation (and error is unavoidable) should never exceed one minute, in order to be assured that we are not mistaken above one degree in the determination of the longitude.*

To one or the other of these five methods persons engaged in this research have chiefly directed their speculations: but there is still a sixth, which seems likewise adapted to the solution of the problem, were it more carefully cultivated; and will perhaps one day furnish us with the most certain method of discovering the longitude; though as yet we are far, very far short of it.

It is not derived from the heavens, but is attached to the earth simply, being founded on the nature of the magnet, and of the compass. The explication of it opens to me a new field of important physical observation, for your amusement and instruction, on the subject of magnetism; and I flatter myself you will attend with delight and improvement to the elucidations which I am going to suggest.

My reflections shall be directed only to the main subject of our present research, I mean the discovery of the longitude. I remark in general, that the magnet is a stone which has the quality of attracting iron, and of disposing itself in a certain direction; and that it communicates the same quality to iron and steel, by rubbing, or simply touching them with a magnet; proposing afterward to enter into a more minute discussion of this quality, and to explain the nature of it.

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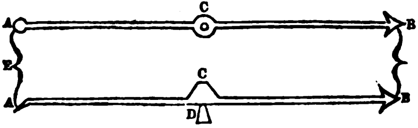
* This method is now brought to very great perfection, not only by the improvement of the lunar tables, but by the perfection of the sextants and circles with which the moon's place in the heavens is observed.—*Ed.*

I begin, then, with the description of a magnetic needle, which, mounted in a certain manner, for the use of mariners, is denominated the *compass*.

For this purpose we provide a needle of good steel, nearly resembling *Fig.* 109, one extremity of

Fig. 109.



which B terminates in a point, the better to distinguish it from the other A; it is furnished at the middle C with a small cap, hollowed below, for the purpose of placing the needle on a pivot or point D, as may be seen in the second figure.

The two ends are adjusted in such a manner, that the needle, being in perfect equilibrium, can revolve freely, or remain at rest, on the pivot, in whatever situation it may be placed. Before the magnet is applied, it would be proper to temper the needle, in order to render it as hard as possible; then by rubbing or touching it with a good loadstone, it will instantly acquire the magnetic virtue. The two extremities will no longer balance each other, but the one B will descend, as if it had become heavier; and in order to restore the equilibrium, something must be taken away from the extremity B, or a small weight added to the end A. But the artists, foreseeing this change produced by magnetism, make the end B originally lighter than the end A, that the

magnetized needle may of itself assume the horizontal position.

It then acquires another property still more remarkable: it is no longer indifferent to all situations as formerly; but affects one in preference to every other, and disposes itself in such a manner that the extremity B is directed to the north nearly, and the extremity A towards the south; and the direction of the magnetic needle corresponds almost with the meridian line.

You recollect that, in order to trace a meridian line, which may point out the north and the south, it is necessary to have recourse to astronomical observations, as the motion of the sun and stars determines that direction; and when we are not provided with the necessary instruments, and especially when the sky is overclouded, it is impossible to derive any assistance from the heavens towards tracing the meridian line; this property of the magnetic needle is, therefore, so much the more admirable, that it points out, at all times, and in every place, the northern direction, on which depends the others, towards the east, south, and west. For this reason the use of the magnetic needle, or compass, is become universal.

It is in navigation that the advantages resulting from the use of the compass are most conspicuous; it being always necessary to direct the course of a vessel towards a certain quarter of the world, in order to reach a place proposed, conformably to geographic or marine charts, which indicate the direction in which we ought to proceed. Before this discovery, accordingly, it was impossible to undertake long voyages; the mariner durst not lose sight of the coast for fear of mistaking his course, unless the sky was unclouded, and the stars pointed out the way.

A vessel on the wide ocean, without the knowledge of the proper course, would be precisely in the state of a man who, with a bandage over his eyes, was obliged to find his way to the great church of Magdeburg; imagining he was going one way, he might be going another. The compass, then, is the principal guide in navigation; and it was not till after this important discovery that men ventured across the ocean, and attempted the discovery of a new world. What would a pilot do without his compass during or after a storm, when he could derive no assistance from the heavens? Take whatever course he might, he must be ignorant in what direction he was proceeding, north, south, or to any other quarter. He would presently deviate to such a degree as infallibly to lose himself. But the compass immediately puts him right; from which you will be enabled to judge of the importance of the discovery of the magnetic needle, or mariner's compass.

6th October, 1761.

LETTER LV.

Declination of the Compass, and Manner of observing it.

Though the magnetic needle affects the situation of being directed from south to north, there are accidental causes capable of deranging this direction, which must be carefully avoided. Such are the proximity of a loadstone, or of iron or steel. You have only to present a knife to a magnetic needle, and it will immediately quit its natural direction, and move towards the knife; and, by drawing the knife round the needle, you will make it assume every possible direction. In order to be assured, then, that the needle is in its natural direction, you must keep at a distance from it all iron or steel, as well as magnets; which is so much the more easy, that these substances influence its direction only when

very near it: once removed, their effect becomes insensible, unless in the case of a very powerful magnet, which might possibly act on the needle at the distance of several feet.

But iron alone produces not this effect, as the compass may be used to advantage even in iron mines. You are perfectly sensible, that under ground, in mines, we are in the same condition as at sea when the face of heaven is overclouded, and that it is necessary to drive mines in a certain direction. Plans are accordingly constructed representing all the tracks hollowed out in the bowels of the earth, and this operation is regulated merely by the compass; this is the object of the science denominated subterraneous geometry.

To return to our compass or magnetic needle: I have remarked that its direction is only almost northerly; it is therefore incorrect to say that the magnet has the property of always pointing north. Having employed myself in the fabrication of many magnetic needles, I constantly found that their direction at Berlin deviated about 15° from the true meridian line; now an aberration of 15° is very considerable.

Fig. 110.

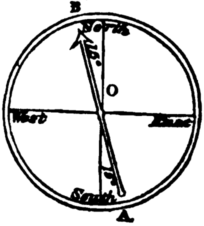


Fig. 110 represents first the true meridian line, drawn from north to south; that which is drawn at right angles with it indicates the east to the right-hand, and the west to the left. Now the magnetic needle A B does not fall on the meridian, but deviates from it an angle of 15° B O *North*. This angle is denominated the *declination*, and sometimes the *deviation* or *variation*, of the compass or magnetic needle; and as the extremity B, nearest the north, deviates towards the west, we say the declination is 15° westerly.

Having thus determined the declination of the magnetic needle, we can make it answer the same purpose as if it pointed directly north. The needle is usually enclosed in a circle, and you have only to mark on it the due north and the exact distance from the northern extremity of the needle, so as to make a declination of 15° westward; and the line *North South*, *Fig.* 110, will indicate the true meridian line, and enable us to ascertain the four cardinal points, north, east, south, and west.

The better to disguise the secret, the magnetic needle is concealed in a circle of pasteboard, as represented in the figure, only the needle is rendered invisible, the pasteboard covering it, and forming but one body with it, the centre of which is placed on a pivot,* in order to admit of a free revolution: it assumes, of course, a situation such that the point marked *North* is always directed to that point of the horizon; whereas the needle, which is not seen, in effect deviates from it 15° to the west. This construction serves only to disguise the declination, which the vulgar consider as a defect, though it be rather an object worthy of admiration, as we shall afterward see; and the pasteboard, only increasing the weight of the needle, prevents its turning so freely as if it were unencumbered.

To remedy this, and more commodiously to employ the compass, the needle is deposited in a circular box, the circumference of which, divided into 360°, exhibits the names of the principal points of the horizon. In the centre is the pivot, or point which supports the needle, and this last immediately assumes a certain direction; the box is then turned till the northern extremity of the needle

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* The cap or hollow which rests on the pivot should be made of garnet, which gives less friction than any other of the precious stones.—Ed.

B exactly corresponds with 15° on the circumference, reckoning from the north-westward; and then the names marked will agree with the real quarters of the world.

At sea, however, they employ needles cased in circles of pasteboard, the circumference of which is divided into 360°, to prevent the necessity of turning round the box; then the pasteboard circle, which is called the compass, indicating the real quarters of the world, we have only to refer to it the course which the ship is steering, in order to ascertain the direction, whether north or south, east or west, or any other intermediate point. By the compass likewise we distinguish the winds, or the quarters from which they blow; and from the points marked on it their names are derived. It is necessary, at any rate, to be perfectly assured of the declination or variation of the compass; we have found it to be exactly 15° westward here at Berlin; but it may be different at other places, as I shall afterward demonstrate.

10*th October*, 1761.

LETTER LVI.

Difference in the Declination of the Compass at the same Place.

When I say that the declination of the compass is 15° west, this is to be understood as applying only to Berlin, and the present time: for it has been remarked, that not only is this declination different at different places of the earth, but that it varies, with time, at the same place.*

The magnetic declination is accordingly much greater at Berlin now than it was formerly. I recollect the time perfectly when it was only 10°;* and in the last century there was a period when there was no declination, so that the direction of the magnetic needle coincided exactly with the meridian line. This was about the year 1670; since then the declination is become progressively greater towards the west, up to 15°, as at this day: and there is every appearance that it will go on diminishing till it is again reduced to nothing. I give this, however, merely as conjecture, for we are very far from being able to predict it with certainty.

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* In the year 1786, *M. Schulze* found the deviation to be 18° 28' which seems to have been its maximum. In 1805, *M. Bode* found it to be 18° 8', having been so low as 17° 5' in 1788.—*Ed.*

Besides, it is well known that prior to the year 1670, the declination was in the contrary direction, that is, towards the east; and the farther back we go, the greater do we find the declination eastward. Now, it is impossible to go farther back than to the period when the compass was discovered; this happened in the fourteenth century; but it was long after the discovery before they began to observe the declination at Berlin; for it was not perceived at first that the needle deviated from the meridian line.

But at London, where this subject has been more carefully studied, the magnetic declination in the year 1580 was observed to be 11° 15′ east; in 1622, 6° 0′ east; in 1634, 4° 5′ east; in 1657 there was no declination; but in 1672 it was 2° 30′ west; in 1692, 6° 0′ west; and at present it may probably be 18° west, or more.† You see, then, that about the beginning of the last century, the declination was nearly 8 degrees east: that thenceforward it gradually diminished, till it became imperceptible in the year 1657; and that it has since become westerly, gradually increasing up to the present time.‡

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* It was so low as 10° at Berlin in 1717.—*Ed.*

† In January, 1821, the variation of the needle at London was 24° 35 west.—*Ed.*

‡ The variation of the magnet is not only different in different countries, but in different places in the same country, situated a few miles

It has preserved nearly the same order at Paris; but there it was reduced to nothing in 1666, nine years later than at London; hence you will observe a most unaccountable diversity of declination relatively to different places of the earth at the same time, and to the same place at different times.

At present, not only through all Europe, but through all Africa, and the greatest part of Asia, the declination is westerly, in some places greater, in others less, than with us. It is greater in certain countries of Europe than at our capital: namely, in Scotland and in Norway, where the declination considerably exceeds 20°; in Spain, Italy, and Greece, on the contrary, it is less, being about 12°; on the western coasts of Africa it is about 10°, and on the eastern 12°. But as you advance eastward into Asia it progressively diminishes, till it entirely disappears in the heart of Siberia, at Jeniseisk; it disappears too in China, at Pekin, and at Japan; but beyond these regions, to the eastward, the declination becomes easterly, and goes on increasing in this direction, along the north part of the Pacific Ocean, to the western coasts of America, from which it proceeds, gradually diminishing, till it again disappears in Canada, Florida, the Antilles, and towards the coasts of Brazil. Beyond these countries, towards the east, that is, towards Europe and Africa, it again becomes westerly, as I have already remarked.

In order to attain a perfect knowledge of the present state of magnetic declination, it would be necessary to ascertain for all places, both at land and sea, the present state of magnetic declination, and whether

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from each other. It is also subject to an hourly change or movement at the same place on the same day, returning generally to the same point, very nearly, at the same hour on each successive day. In the year 1830 agreeably to *Professor Fisher*, the variation at New-Haven was 4° 25′ 25″ W. The annual variation is 2′ 49″: so that the needle appears to be gradually advancing towards the true meridian, after which it will probably acquire an easterly variation.—*Am. Ed.*

its tendency is westward or eastward. This knowledge would be undoubtedly extremely useful, but we dare scarcely hope for it. It would require men of ability in every part of the globe, employed at the same time in observing, each on his own station, the magnetic declination, and who should communicate their observations with the utmost exactness. But the space of some years would elapse before the communications of the more remote could be received, thus the knowledge aimed at is unattainable till after the expiration of years. Now, though no very considerable change takes place in the direction of the magnetic needle in two or three years, this change, however small, would prevent the attainment of complete information respecting the present state of the various declination of the magnetic needle, from observations made at the same time in the different regions of the globe.

The same thing holds with respect to times past; to every year corresponds a certain state of magnetic declination proper to itself, and which distinguishes it from every other period of time, past and future. It were, however, sincerely to be wished that we had an exactly detailed state of the declination for one year only; the most important elucidations of the subject would certainly be derived from it.

The late *Mr. Halley*, a celebrated English astronomer, has attempted to do this for the year 1700, founding his conclusions on a great number of observations made at different places, both by land and sea; but, besides that some very considerable districts, where these observations were not made, are not taken into his account, most of those which he has employed were made several years prior to 1700; so that at this era the declination might have undergone very considerable alterations. It follows that this statement, which we find represented on a general chart of the earth, must be considered as ex-

tremely defective; and, moreover, what would it now avail us to know the state of magnetic declination for the year 1700, having since that time undergone a considerable change?

Other English geographers have produced, posterior to that period, a similar chart, intended to represent all the declinations such as they were in the year 1744. But as it has the same defect with that of *Mr. Halley*, and as they likewise were unable to procure observations from several countries on the globe, they did not scruple to fill up the vacant places by consulting *Halley's* chart, which certainly could not apply to 1744. You will conclude, from what I have said, that our knowledge of this important branch of physics is extremely imperfect.*

13th October, 1761.

LETTER LVII.

Chart of Declinations; Method of employing it for the Discovery of the Longitude.

It may be proper likewise to explain in what manner *Halley* proceeded to represent the magnetic declinations in the chart which he constructed for the year 1700, that if you should happen to see it, you may comprehend its structure.

First, he marked at every place the declination of the magnetic needle, such as it had been there observed. He distinguished, among all these places, those where there was no declination, and found that they all fall in a certain line, which he calls the *line of no declination*, as everywhere under that line there was then none. This line was neither a meridian nor a parallel, but ran in a very oblique direction over North America, and left it near the coasts of Carolina; thence it bent its course across the Atlantic Ocean, between Africa and America. Besides this line, he discovered likewise another in which the declination disappeared; it descended through the middle of China, and passed from thence through the Philippine Isles and New-Holland. It is easy to judge, from the track of these two lines, that they have a communication near both poles of the globe.

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* Very correct and interesting charts, both of the variation and the dip of the magnetic needle, have been recently constructed by *Mr. Hansteen* of Christiania in Norway, and published in his very able work on the Magnetism of the Earth. *Mr. Hansteen's* charts will be found in the *Edinburgh Philosophical Journal*, vol. iv. p. 303.—*Ed.*

Having fixed these two lines of no declination, *Mr. Halley* remarked that everywhere between the first and last, proceeding from west to east, that is, through all Europe, Africa, and almost the whole of Asia, the declination was westerly; and that on the other side, between those lines, that is, over the whole Pacific Ocean, it was easterly. After this, he observed all the places in which the declination was 5 degrees west, and found he could still conveniently draw a line through all these places, which he calls the *line of five degrees west.* He found likewise two lines of this description, the one of which accompanied, as it were, the first of no declination, and the other the last. He went on in the same manner with the places where the declination was 10°; afterward 15°, 20°, &c.; and he saw that these lines of great declination were confined to the polar regions; whereas those of small declination encompassed the whole globe, and passed through the equator.

In fact, the declination scarcely ever exceeds 15° on the equator, whether west or east; but on approaching the poles, it is possible to arrive at places where the declination exceeds 58° and 60°. There are undoubtedly some where it is still greater, exceeding even 90°, and where the northern extremity of the needle will consequently turn about and point southward.*

Finally, having drawn similar lines through the places where the declination was eastward 10°, 15°, 20°, and so on, *Mr. Halley* filled up the whole chart, which represented the entire surface of the earth, under each of which lines the declination is universally the same, provided the observations are not erroneous. *Mr. Halley* has accordingly scrupulously abstained from continuing such lines beyond the places where observations had actually been made: for this reason the greater part of his chart is a blank.

Had we such a chart accurate and complete, we should see at a glance what declination must have predominated at each place at the time for which the chart was constructed; and though the place in question should not be found precisely under one of the lines traced on the chart, by comparing it with the two lines between which it might be situated, we could easily calculate the intermediate declination which corresponds to it. If I found my present place to be between the lines of 10° and 15° of western declination, I should be certain that the declination there was more than 10°, and less than 15°; and according as I might be nearer the one or the other, I could easily find the means which would indicate the true declination.

From this you will readily comprehend, that if we had such a chart thus exact, it would assist us in discovering the longitude, at least for the time to which it corresponded. In order to explain this method, let us suppose that we are possessed of a chart constructed for the present year, we would see on it, first, the two lines drawn through the places where there is no declination; then the two where it is 5°, 10°, 15°, 20°, both east and west: let us further suppose that, for the greater exactness, these lines were drawn from degree to degree, and that I found myself at a certain place on sea, or in an unknown country, I would in the first place draw a meridian line, in order to ascertain how much my compass deviated from it, and I should find, for example, that the declination is precisely 10° east; I should then take my chart, and look for the two lines under which the declination is 10° east, fully assured that I am under the one or the other of these two lines, which must at once greatly relieve my uncertainty. Finally, I would observe the height of the pole, which being the latitude of my place, nothing more would remain but to mark, on the two lines mentioned, the points where the latitude is the same with that which I have just observed, and then all my uncertainty is reduced to two points very distant from each other; now the circumstances of my voyage would easily determine which of those two places is that where I actually am.

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* This was found to be the case in the voyages of *Captain Ross* and *Captain Parry*. On the S.E. point of Byam Martin's Island, in west lon. 103° 44½′, and north lat. 75° 9′, the variation was 165° 50′ east, having been 128° 50′ west in west lon. 91° 47′, and north lat. 74° 40′.—*Ed.*

You will admit that if we had charts such as I have described, this method would be the most commodious and accurate of all for ascertaining the longitude; but this is precisely the thing we want; and as we are still very far from having it in our power to construct one for the time past, which would be of no use for the present time, for want of a sufficient number of observations, we are still less instructed respecting all the changes of declination which every place undergoes in the lapse of time. The observations hitherto made assure us that certain places are subject to very considerable variations, and that others scarcely undergo any, in the same interval of time; which strips us of all hope of ever being able to profit by this method, however excellent it may be in itself.

17th October, 1761.

LETTER LVIII.

Why does the Magnetic Needle affect, in every Place of the Earth, a certain Direction, differing in different Places; and for what Reason does it change, with Time, at the same Place?

You will undoubtedly have the curiosity to be informed why magnetic needles affect, at every place on the globe, a certain direction; why this direction is not the same at different places; and why, at the same place, it changes with the course of time. I shall answer these important inquiries to the best of my ability, though, I fear, not so much to your satisfaction as I could wish.

I remark, first, that magnetic needles have this property in common with all magnets, and that it is only their form, and their being made to balance and revolve freely on a pivot, which renders it more conspicuous. The loadstone, suspended by a thread, turns towards a certain quarter, and when put in a small vessel to make it swim on water, the vessel which supports the loadstone will always affect a certain direction. Every loadstone fitted with two opposite points, the one of which is directed to the north, and the other to the south, will be subject to the same variations as the magnetic needle.

These points are very remarkable in all loadstones, as by them iron is attracted with the greatest force.

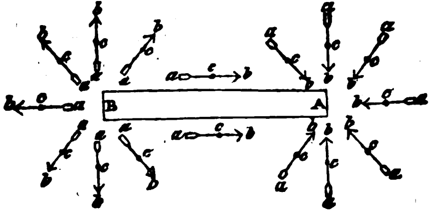
They are denominated the *poles* of a loadstone—a term borrowed from that of the poles of the earth, or of the heavens; because the one has a tendency towards the north, and the other towards the south pole of the earth: but this is to be understood as only almost, not exactly, the case; for when the name was imposed, the declination had not yet been observed. That pole of the loadstone which is directed northward is called its north pole, and that which points southward its south pole.

I have already remarked, that a magnetic needle, as well as the loadstone itself, assumes this situation, which appears natural to it only when removed from the vicinity of another loadstone, or of iron. When a magnetic needle is placed near a loadstone, its situation is regulated by the poles of that loadstone: so that the north pole of the loadstone attracts the southern extremity of the needle; and reciprocally, the south pole of the loadstone the northern extremity of the needle. For this reason, in referring one loadstone to another, we call those the friendly poles which bear different names, and those the hostile which have the same name. This property is singularly remarkable on bringing two loadstones near each other; for then we find, that not only do the poles of different names mutually attract, but that those of the same name shun and repel each other. This is still more conspicuous when two magnetic needles are brought within the sphere of mutual influence.

In order to be sensible of this, it is of much importance to consider the situation which a magnetic needle assumes in the vicinity of a loadstone.

The bar AB, *Fig.* 111, represents a loadstone,

Fig. 111.



whose north pole is B, and the south pole A: you see various positions of the magnetic needle, under the figure of an arrow, whose extremity marked *b* is the north pole, and *a* the south. In all these positions, the extremity *b* of the needle is directed towards the pole A of the loadstone; and the extremity *a* to the pole B. The point *c* indicates the pivot on which the needle revolves; and you have only to consider the figure with some attention in order to determine what situation the needle will assume, in whatever position round the loadstone the pivot *c* is fixed.

If there were, therefore, anywhere a very large loadstone AB, the magnetic needles placed round it would assume at every place a certain situation, as we see actually to be the case round the globe. Now if the globe itself were that loadstone, we should comprehend why the magnetic needles everywhere assumed a certain direction. Naturalists, accordingly, in order to explain this phenomenon, maintain that the whole globe has the property of a magnet, or that we ought to consider it as a prodigious loadstone. Some of them allege, that there is at the centre of the earth a very large loadstone, which has exercised its influence on all the magnetic needles, and even on all the loadstones, which are to be found on the surface of the earth; and that it is this influence which directs them in every place, conformably to the directions which we observe them to assume.

But there is no occasion to have recourse to a loadstone concealed in the bowels of the earth. Its surface is so replenished with mines of iron and loadstone, that their united force may well supply the want of this huge magnet. In fact, all loadstones are extracted from mines—an infallible proof that these substances are found in great abundance in the bowels of the earth, and that the union of all their powers furnishes the general force which produces all the magnetical phenomena. We are likewise enabled thereby to explain why the magnetic declination changes, with time, at the same place; for it is well known that mines of every kind of metal are subject to perpetual change, and particularly those of iron, to which the loadstone is to be referred. Sometimes iron is generated, and sometimes it is destroyed at one and the same place; there are accordingly at this day mines of iron where there were none formerly; and where it was formerly found in great abundance there are now hardly any traces of it. This is a sufficient proof that the total mass of loadstones contained in the earth is undergoing very considerable changes, and thereby undoubtedly the poles, by which the magnetic declination is regulated, likewise change with the lapse of time.

Here, then, we must look for the reason why the magnetic declination is subject to changes so considerable at the same place of the globe. But this very reason, founded on the inconstancy of what is passing in its bowels, affords no hope of our ever being able to ascertain the magnetic declination beforehand, unless we could find the means of subjecting the changes of the earth to some fixed law. A long series of observations, carried on through several ages successively, might possibly throw some light on the subject.

20th October, 1761.

LETTER LIX.

Elucidations respecting the Cause and Variation of the Declination of Magnetic Needles.

Those who allege that the earth contains in its womb a prodigious loadstone, like a stone with a kernel in fruit, are under the necessity of admitting, in order to explain the magnetic declination, that this stone is successively shifting its situation. It must in that case be detached from the earth in all its parts; and as its motion would undoubtedly follow a certain law, we might flatter ourselves with the hope of one day discovering it. But whether there be such a magnetic stone within the earth, or whether the loadstones scattered up and down through its entrails unite their force to produce the magnetical phenomena, we may always consider the earth itself as a loadstone, in subserviency to which every particular loadstone, and all magnetic needles, assume their direction.

Certain naturalists have enclosed a very powerful magnet in a globe, and having placed a magnetic needle on its surface, observed phenomena similar to those which take place on the globe of the earth, by placing the magnet within the globe in several different positions. Now, considering the earth as a loadstone, it will have its magnetic poles, which must be carefully distinguished from the natural poles round which it revolves. These poles have nothing in common between them but the name; but it is from the position of the magnetic poles relatively to the natural that the apparent irregularities in the magnetic declination proceed, and particularly of the lines traced on the globe, of which I have endeavoured to give you some account.

In order more clearly to elucidate this subject, I remark, that if the magnetic poles exactly coincided with the natural, there would be no declination all over the earth; magnetic needles would universally point to the north precisely, and their position would be exactly that of the meridian line. This would no doubt be an unspeakable advantage in navigation, as we should then know with precision the course of the vessel and the direction of the wind; whereas at present we must always look for the declination of the compass before we are able to determine the true quarters of the world. But then the compass could furnish no assistance towards ascertaining the longitude, an object which the declination may sooner or later render attainable.

Hence it may be concluded, that if the magnetic poles of the earth differed very greatly from the natural, and that if they were directly opposite to each other—which would be the case if the magnetic axis of the earth, that is, the straight line drawn from the one magnetic pole to the other, passed through the centre of the earth—then magnetic needles would universally point towards these magnetic poles, and it would be easy to assign the magnetic direction proper to every place; we should only have to draw for every place a circle which should at the same time pass through the two magnetic poles, and the angle which this circle would make with the meridian of the same place must give the magnetic declination.

In this case, the two lines under which there is no declination would be the meridians drawn through the magnetic poles. But as we have seen that, in reality, these two lines without declination are not meridians, but take a very unaccountable direction, it is evident that no such case actually takes place. *Halley* clearly saw this difficulty, and therefore thought himself obliged to suppose a double loadstone in the bowels of the earth, the one fixed, the other moveable; of consequence, he was obliged to admit four poles of the earth, two of them towards the north and two towards the south, at unequal distances. But this hypothesis seems to me rather a bold conjecture: it by no means follows, that because these lines of no-declination are not meridians, there must be four magnetic poles on the earth; but rather, that there are only two, which are not directly opposite to each other; or, which comes to the same thing, that the magnetic axis does not pass through the centre of the earth.

It remains, therefore, that we consider the cases in which these two magnetic poles are not directly opposite, and in which the magnetic axis does not pass through the centre of the earth; for if we embrace the hypothesis of the magnetic nucleus within the earth, why should one of its poles be precisely opposite to the other? This nucleus may very probably be not exactly in the very centre of the earth, but at a considerable distance from it. Now, if the magnetic poles are not diametrically opposite to each other, the lines of no declination may actually assume a direction similar to that which, from observation, we find they do; it is even possible to assign to the two magnetic poles such places on the earth, that not only these lines should coincide with observation, but likewise, for every degree of declination, whether western or eastern, we may find lines precisely similar to those which at first seemed so unaccountable.

In order, then, to know the state of magnetic declination, all that is requisite is to fix the two magnetic poles; and then it becomes a problem in geometry to determine the direction of all the lines which I mentioned in my preceding Letter, drawn for every place where the declination is the same: by such means, too, we should be enabled to rectify these lines, and to fill up the countries where no observations have been made; and were it possible to assign, for every future period, the places of the two magnetic poles on the globe, it would undoubtedly prove the most satisfactory solution of the problem of the longitude.

There is no occasion, therefore, for a double loadstone within the earth, or for four magnetic poles, in order to explain the declination of magnetic needles, as *Halley* supposed; but for a simple magnet, or two magnetic poles, provided its just place is assigned to each.* It appears to me, that, from this reflection, we are much more advanced in our knowledge o magnetism.

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* The phenomena render it absolutely necessary to admit two mag-

24th October, 1761.

LETTER LX.

Inclination or Dip of Magnetic Needles.

You will please to recollect, that on rubbing a needle against the loadstone, it acquires not only the property of pointing towards a certain point of the horizon, but that its northern extremity sinks, as if it had become heavier, which obliges us to diminish its weight somewhat, or to increase that of the other extremity, in order to restore the equilibrium. I have, without putting this in practice, made several experiments to ascertain how far the magnetic force brought down the northern extremity of the magnetized needle, and I have found that it sank so as to make an angle of 72 degrees with the horizon, and that in this situation the needle remained at rest. It is proper to remark, that these experiments were made at Berlin about six years ago; for I shall show you afterward, that this direction to a point below the horizon is as variable as the magnetic declination.

Hence we see that the magnetic power produces a double effect on needles; the one directs the needle

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netic poles. The two northern poles, which we may call B and *b*, and the two southern poles, A and *a*, were thus situated, according to *Hansteen*, in 1823.

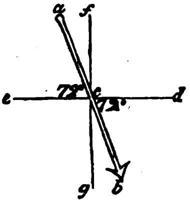
	North Lat.		West Long.	
B in	69° 34′	and	271° 38′	from Greenwich.
b	85 9		142 11	
A	68 48		132 11	
a	78 23		223 8	

	Years.
The pole B moves round the north pole of the globe in	1740.
——— *b*, which is weaker than B, in	860.
——— A moves round the south pole of the globe in	4609.
——— *a*, which is weaker than A, in	1304.

See the *Edinburgh Philosophical Journal*, vol. iv. p. 117.—*Ed.*

towards a certain quarter of the horizon, the deviation of which from the meridian line is what we call the magnetic declination; the other impresses on it an inclination towards the horizon, sinking the one or the other extremity under it up to a certain angle.

Fig. 112.



Let *d e*, *Fig.* 112, be the horizontal line, drawn according to the magnetic declination, and the needle will assume, at Berlin, the situation *b a*, which makes with the horizon *d e* the angle *d c b*, or *e c a*, which is 72°, and consequently with the vertical *f g* an angle *b c g*, or *a c f*, of 18°. This second effect of the magnetic force, by which the magnetic needle affects a certain inclination towards the horizon, is as remarkable as the first; and as the first is denominated the magnetic *declination*, the second is known by the name of magnetic *inclination* or *dip*, which deserves, as well as the declination, to be everywhere observed with all possible care, as we find in it a similar variation.

The inclination at Berlin has been found 72°,* at Bâsle only 70°, the northern extremity of the needle being sunk, and the opposite, of consequence, raised to that angle. This takes place in countries which are nearer to the northern magnetic pole of the earth; and in proportion as we approach it, the greater becomes the inclination of the magnetic needle, or the more it approaches the vertical line; so that if we could reach the magnetic pole itself, the needle would there actually assume a vertical situation; its northern extremity pointing perpendicularly downwards, and its southern end upwards.†

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* In 1805, it was found by *Humboldt* to be 69° 53′ at Berlin.—*Ed.*

† On the 18th July, 1820, the inclination of the needle was observed by *Mr. Sabine* to be 88° 43′ 5″ at Winter Harbour in Melville Island, in west lougitude 110° 48′, and 74° 47′ of north latitude.—*Ed.*

The farther, on the contrary, you remove from the northern magnetic pole of the earth, and approach the southern, the more the inclination diminishes; it will at length disappear, and the needle will assume a horizontal position, when equally distant from both poles; but in proceeding towards the south pole of the earth, the southern extremity of the needle will sink more and more under the horizon, the northern extremity rising in proportion, till at the pole itself the needle again becomes vertical, with the southern extremity perpendicularly downwards, and the northern upwards.

It were devoutly to be wished that experiments had been as carefully and as generally made, with the view of ascertaining the magnetic inclination as of determining the declination; but this important article of experimental philosophy has hitherto been too much neglected, though certainly neither less curious nor less interesting than that of the declination. This is not, however, a matter of surprise: experiments of this sort are subject to too many difficulties; and almost all the methods hitherto attempted of observing the magnetic inclination have failed. One artist alone, *Mr. Diterich*, of Bâsle, has succeeded, having actually constructed a machine proper for the purpose, under the direction of the celebrated *Mr. Daniel Bernouilli*. He sent me two of the machines, by means of which I have observed, at Berlin, this inclination of 72 degrees; and however curious in other respects the English and French may be in prosecuting such inquiries, they have put no great value on *Mr. Diterich's* machine, though it is the only one adapted for this purpose.*

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* One of the simplest machines for measuring the dip of the needle is *Capt. Scoresby's* magnetimeter. A bar of iron deprived entirely of its magnetism, either by heat, or by hammering it in the magnetic equator, is placed in the magnetic meridian, upon an inclined plane. This plane is raised or depressed by a wheel and pinion, till the iron bar exercises no action whatever upon a compass-needle placed near it. When this happens, the bar is in the magnetic equator, and consequently the com-

This instance demonstrates how the progress of science may be obstructed by prejudice; hence Berlin and Bâsle are the only two places on the globe where the magnetic inclination is known.

Needles prepared for the construction of compasses are by no means proper to indicate the quantity of magnetic inclination, though they may convey a rough idea of its effect, because the northern extremity in these latitudes becomes heavier. In order to render serviceable needles intended to discover the declination, we are under the necessity of destroying the effect of the inclination, by diminishing the weight of the northern extremity, or increasing that of the southern. To restore the needle to a horizontal position, the last of these methods is usually employed, and a small morsel of wax is affixed to the southern extremity of the needle. You are abundantly sensible that this remedy applies only to these regions of the globe where the inclinatory power is so much, and no more; and that were we to travel with such a needle towards the northern magnetic pole of the earth, the inclinatory power would increase, so that to prevent the effect we should be obliged to increase the quantity of wax at the southern extremity. But were we travelling southward, and approaching the opposite pole of the earth, where the inclinatory power on the northern extremity of the needle diminishes, the quantity of wax affixed to the other extremity must then likewise be diminished; after that it must be taken away altogether, being wholly useless when we arrive at places where the magnetic inclination disappears. On proceeding still forward to the south pole, the southern extremity of the needle sinks; so

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plement of the inclination of the plane on which it rests is the dip or inclination of the needle at the place where the observation is made. This angle of inclination was measured by a vertical graduated circle, adjusted to zero when the bar had a horizontal position.—See the *Edinburgh Transactions*, vol. ix., and the *Edinburgh Philosophical Journal*, vol. ix. p. 42, for a full account of this instrument.—*Ed.*

that to remedy this, a morsel of wax must be affixed to the northern extremity of the needle. Such are the means employed in long voyages to preserve the compass in a horizontal position.

In order to observe the magnetic inclination, it would be necessary to have instruments made on purpose, similar to that invented by the artist of Basle. His instrument is called the *inclinatory;* but there is little appearance of its coming into general use. It is still less to be expected that we should soon have charts constructed with the magnetic inclination, similar to those which represent the declination. The same method might easily be followed, by drawing lines through all the places where the magnetic inclination is the same: so that we should have lines of no inclination; afterward other lines where the inclination would be 5°, 10°, 15°, 20°, and so on, whether northward or southward.*

27th October, 1761.

LETTER LXI.

True Magnetic Direction; subtile Matter which produces the Magnetic Power.

In order to form a just idea of the effect of the earth's magnetic power, we must attend at once to the declination and inclination of the magnetic needle, at every place of the globe. At Berlin, we know the declination is 15° west, and the inclination of the northern extremity 72°. On considering this double effect, the declination and inclination, we shall have the true magnetic direction for Berlin. We draw first, on a horizontal plane, a line which shall make with the meridian an angle of 15° west, and thence descending towards the vertical line, we

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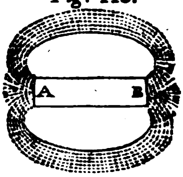
* See Note on Letter LVI.

trace a new line, which shall make with it an angle of 72°; and this will give us the magnetic direction for Berlin: from which you will comprehend how the magnetic direction for every other place is to be ascertained, provided the inclination and declination are known.

Every magnet exhibits phenomena altogether similar. You have only to place one on a table covered with filings of steel, and you will see the filings arrange themselves round the loadstone A B, nearly as represented in *Fig.* 113, in which every particle of the filings may be considered as a small magnetic needle, indicating at every point round the loadstone the magnetic direction. This experiment leads us to inquire into the cause of all these phenomena.

Fig. 113.



The arrangement assumed by the steel filings leaves no room to doubt that it is a subtile and invisible matter which runs through the particles of the steel, and disposes them in the direction which we here observe. It is equally clear that this subtile matter pervades the loadstone itself, entering at one of the poles, and going out at the other, so as to form, by its continual motion round the loadstone, a vortex which reconducts the subtile matter from one pole to the other; and this motion is, without doubt, extremely rapid.

The nature of the loadstone consists, then, in a continual vortex, which distinguishes it from all other bodies; and the earth itself, in the quality of a loadstone, must be surrounded with a similar vortex, acting everywhere on magnetic needles, and making continual efforts to dispose them according to its own direction, which is the same I formerly denominated the magnetic direction: this subtile matter is continually issuing at one of the magnetic poles of the earth, and after having performed a circuit round to the other pole, it there enters, and pervades the globe through and through to the opposite pole, where it again escapes.

We are not yet enabled to determine by which of the two magnetic poles of the earth it enters or issues; the phenomena depending on this have such a perfect resemblance, that they are indistinguishable. It is undoubtedly, likewise, this general vortex of the globe which supplies the subtile matter of every particular loadstone to magnetic iron or steel, and which keeps up the particular vortices that surround them.

Previous to a thorough investigation of the nature of this subtile matter, and its motion, it must be remarked, that its action is confined to loadstone, iron, and steel;* all other bodies are absolutely indifferent to it: the relation which it bears to those must therefore be by no means the same which it bears to others. We are warranted to maintain, from manifold experiments, that this subtile matter freely pervades all other bodies, and even in all directions. for when a loadstone acts upon a needle, the action is perfectly the same whether another body interposes or not, provided the interposing body is not iron, and its action is the same on the filings of iron. This subtile matter, therefore, must pervade all bodies, iron excepted, as freely as it does air, and even pure ether; for these experiments succeed equally well in a receiver exhausted by the air-pump. This matter is consequently different from ether, and even much more subtile. And, on account of the general vortex of the earth, it may be affirmed that the globe is completely surrounded by it, and

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* Professor Hansteen has lately found that every vertical object, of whatever materials it is composed, has a *magnetic south pole above*, and a *magnetic north pole below*. This curious fact he has put beyond a doubt, by measuring the velocity of the oscillations of a magnetic needle on different sides of the extremities of the vertical object.—See the *Edinburgh Philosophical Journal*, vol. iv. p. 299, 300.—*Ed.*

freely pervaded, as all other bodies are, excepting the loadstone and iron; for this reason iron and steel may be denominated magnetic bodies, to distinguish them from others.

But if this magnetic matter passes freely through all non-magnetic bodies, what relation can it have to those which are such? We have just observed, that the magnetic vortex enters at one of the poles of every loadstone, and goes out at the other; whence it may be concluded that it freely pervades loadstones likewise, which would not distinguish them from other bodies. But as the magnetic matter passes through the loadstone only from pole to pole, this is a circumstance very different from what takes place in others. Here, then, we have the distinctive character. Non-magnetic bodies are freely pervaded by the magnetic matter in all directions: loadstones are pervaded by it in one direction only; one of the poles being adapted to its admission, the other to its escape. But iron and steel, when rendered magnetic, fulfil this last condition; when they are not, it may be affirmed that they do not grant a free transmission to the magnetic matter in any direction.

This may appear strange, as iron has open pores, which transmit the ether, though it is not so subtile as the magnetic matter. But we must carefully distinguish a simple passage, from one in which the magnetic matter may pervade the body, with all its rapidity, without encountering any obstacle.

31st October, 1761.

LETTER LXII.

Nature of the Magnetic Matter, and of its rapid Current. Magnetic Canals.

I AM very far from pretending to explain perfectly the phenomena of magnetism; it presents difficulties which I did not find in those of electricity. The cause of it undoubtedly is, that electricity consists in too great or too small a degree of compression of a subtile fluid which occupies the pores of bodies, without supposing that subtile fluid, which is the ether, to be in actual motion: but magnetism cannot be explained unless we suppose a vortex in rapid agitation, which penetrates magnetic bodies.

The matter which constitutes these vortices is likewise much more subtile than ether, and freely pervades the pores of loadstones, which are impervious even to ether. Now, this magnetic matter is diffused through and mixed with the ether, as the ether is with gross air; or, just as ether occupies and fills up the pores of air, it may be affirmed that the magnetic matter occupies and fills the pores of ether.

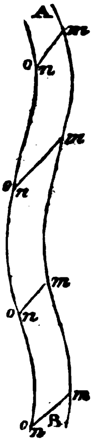
I conceive, then, that the loadstone and iron have pores so small that the ether in a body cannot force its way into them, and that the magnetic matter alone can penetrate them: and which, on being admitted, separates itself from the ether by what may be called a kind of filtration. In the pores of the loadstone alone, therefore, is the magnetic matter to be found in perfect purity: everywhere else it is blended with ether, as this last is with the air.

You can easily imagine a series of fluids, one always more subtile than another, and which are perfectly blended together. Nature furnishes instances of this. Water, we know, contains in its pores particles of air, which are frequently seen discharging themselves in the form of small bubbles: air again, it is equally certain, contains in its pores a fluid incomparably more subtile—namely, ether—and which on many occasions is separated from it, as in electricity. And now we see a still further progression, and that ether contains a matter much more subtile than itself—the magnetic matter—which may

perhaps contain, in its turn, others still more subtile; at least this is not impossible.

Having considered the nature of this magnetic matter, let us see how the phenomena are produced. I consider a loadstone, then; and say, first, that besides a great many pores filled with ether, like all other bodies, it contains some still much more narrow, into which the magnetic matter alone can find admission. Secondly, these pores are disposed in such a manner as to have a communication with each other, and constitute tubes or canals, through which the magnetic matter passes from the one extremity to the other. Finally, this matter can be transmitted through these tubes only in one direction, without the possibility of returning in an opposite direction. This most essential circumstance requires a more particular elucidation.

Fig. 114.



First, then, I remark, that the veins and lymphatic vessels in the bodies of animals are tubes of a similar construction, containing valves, represented in *Fig.* 114, by the strokes *m n*, which, by raising themselves, grant a free passage to the blood when it flows from A to B, and to prevent its reflux from B to A. For if the blood attempted to flow from B to A, it would press down the moveable extremity of the valve *m* on the side of the vein *o*, and totally obstruct the passage. Valves are thus employed in aqueducts, to prevent the reflux of the water. I do not consider myself, then, as supposing any thing contrary to nature, when I say that the canals in loadstones, which admit the magnetic matter only, are of the same construction.

Fig. 115.

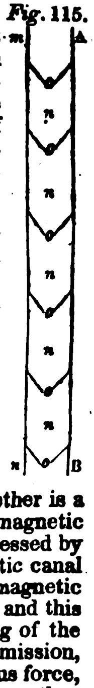


Fig. 115, represents this magnetic canal, according to my idea of it. I conceive it furnished inwardly with bristles directed from A towards B, which present no opposition to the magnetic matter in its passage from A to B, for in this case they open of themselves at *n*, to let the matter pass at *o;* but they would immediately obstruct the channel were it to attempt a retrograde course from B to A. The nature of magnetic canals consists, then, in granting admission to the magnetic matter only at A, to flow towards B, without the possibility of returning in the opposite direction from B towards A.

This construction enables us to explain how the magnetic matter enters into these tubes, and flies through them with the greatest rapidity, even when the whole ether is in a state of perfect rest, which is the most surprising; for how can a motion so rapid be produced? This will appear perfectly clear to you, if you will please to recollect that ether is a matter extremely elastic; accordingly, the magnetic matter, which is scattered about, will be pressed by it on every side. Let us suppose the magnetic canal A B still quite empty, and that a particle of magnetic matter *m* presents itself at the entrance A; and this particle pressed on every side at the opening of the canal, into which the ether cannot force admission, it will there be pressed forward with prodigious force, and enter into the canal with equal rapidity: another particle of magnetic matter will immediately present itself, and be driven forward with the same force; and in like manner all the following particles. There will thence result a continual flux of magnetic matter, which, meeting with no obstruction in this canal, will escape from it at B with the same rapidity that it enters at A.

My idea then is, that every loadstone contains a great multitude of these canals, which I denominate magnetic; and it very naturally follows, that the magnetic matter dispersed in the ether must enter into them at one extremity, and escape at the other, with great impetuosity; that is, we shall have a perpetual current of magnetic matter through the canals of the loadstone: and thus I hope I have surmounted the greatest difficulties which can occur in the theory of magnetism.

3*d November*, 1761.

LETTER LXIII.

Magnetic Vortex. Action of Magnets upon each other.

You have now seen in what the distinctive character of the loadstone consists; and that each contains several canals, of which I have attempted to give a description.

Fig. 116.

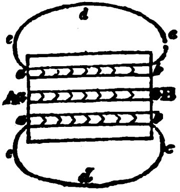


Fig. 116 represents a loadstone A B, with three magnetic canals *a b*, through which the magnetic matter will flow with the utmost rapidity, entering at the extremities marked *a*, and escaping at those marked *b*: it will escape indeed with the same rapidity; but immediately meeting with the ether blended with the grosser air, great obstructions will oppose the continuation of its motion in the same direction; and not only will the motion be retarded, but its direction diverted towards the sides *c c*. The same thing will take place at the entrance, towards the extremities *a a a*; on account of the rapidity with which the particles of magnetic matter force their way into them, the circulation will quickly overtake those which are still towards the sides *e e*, and these in their turn will be replaced by those which, escaping from the extremities *b b b*, have been already diverted towards *c c*; so that the same magnetic matter which issued from the extremities *b b b* quickly returns towards those marked *a a a*, performing the circuit *b c d e a*; and this circulation round the loadstone is what we call the *magnetic vortex*.

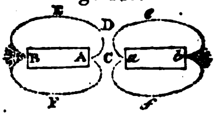
It must not be imagined, however, that it is always the same magnetic matter which forms these vortices: a considerable part of it will escape, no doubt, as well towards B as towards the sides, in performing the circuit; but as a compensation, fresh magnetic matter will enter by the extremities *a a a*, so that the matter which constitutes the vortex is succedaneous and very variable: a magnetic vortex, surrounding the loadstone, will, however, always be kept up, and produce the phenomena formerly observed in filings of steel scattered round the loadstone.

You will please further to attend to this circumstance, that the motion of the magnetic matter in the vortex is incomparably slower out of the loadstone than in the magnetic tubes, where it is separated from the ether, after having been forced into them by all the elastic power of this last fluid; and that on escaping it mixes again with the ether, and thereby loses great part of its motion, so that its velocity in travelling to the extremities *a a a* is incomparably less than in the magnetic canals *a b*, though still very great with respect to us. You will easily comprehend, then, that the extremities of the magnetic canals, by which the matter enters into the loadstone and escapes from it, are what we call its poles; and that the magnetic poles of a loadstone are by no means mathematical points, the whole space in which the extremities of the magnetic canals terminate being one magnetic pole, as in the loadstone represented by *Fig.* 113 (p. 212), where the whole surfaces A and B are the two poles.

Now, though these poles are distinguished by the terms *north* and *south*, yet we cannot affirm with certainty whether it is by the north or south pole that the magnetic matter enters into loadstones. You will see, in the sequel, that all the phenomena produced by the admission and escape have such a perfect resemblance that it appears impossible to determine the question by experiments. It is therefore a matter of indifference whether we suppose that the magnetic matter enters or escapes by the north pole or by the south.

Be this as it may, I shall mark with the letter A the pole by which the magnetic matter enters, and with B that by which it escapes, without pretending thereby to indicate which is north or south. I proceed to the consideration of these vortices, in order to form a judgment how two loadstones act upon each other.

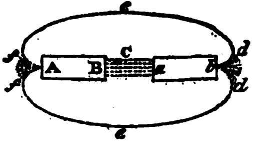
Fig. 117.



Let us suppose that the two loadstones A B and *a b*, *Fig.* 117, are presented to each other by the poles of the same name A, *a*, and their vortices will be in a state of total opposition. The magnetic matter which is at C will enter at A and *a*, and these two vortices attempting mutually to destroy each other, the matter which proceeds by E to enter at A will meet at D that of the other loadstone returning by *e* to enter at *a*: from this must result a collision of the two vortices, in which the one will repel the other; and this effect will extend to the loadstones themselves, which, thus situated, undergo mutual repulsion. The same thing would take place if the two loadstones presented to each other the other poles B and *b*: for this reason the poles of the same name are denominated *hostile*, because they actually repel each other.

But if the loadstones present to each other the poles of a different name, an opposite effect will ensue, and you will perceive that they have a mutual attraction.

Fig. 118.



In *Fig.* 118, where the two loadstones present to each other the poles B and *a*, the magnetic matter which issues from the pole B, finding immediately free admission into the other loadstone by its pole *a*, will not be diverted towards the sides in order to return and re-enter at A, but will pass directly by C into the other loadstone, and escape from it at *b*, and will perform the circuit by the sides *d d*, to re-enter, not by the pole *a*, but by the pole A, of the other loadstone, completing the circuit by *e f*. Thus the vortices of these two loadstones will unite, as if there were but one; and this vortex, being compressed on all sides by the ether, will impel the two loadstones towards each other, so that they will exhibit a mutual attraction.

This is the reason why the poles of different names are denominated *friendly*, and those of the same name *hostile*, the principal phenomenon in magnetism, in as much as the poles of different names attract, and those of the same name repel each other.

7th November, 1761.

LETTER LXIV.

Nature of Iron and Steel. Method of communicating to them the Magnetic Force.

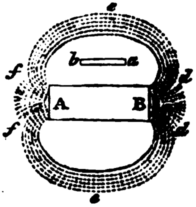
Having settled the nature of the loadstone in these canals which the magnetic matter can pervade in only one direction, because the valves they contain prevent its return in the contrary direction, you can no longer doubt that they are the continuation of those pores (see *Fig.* 115, p. 217), whose fibres point in the same direction; so that several of these particles, being joined in continuation, constitute one magnetic canal. It is not sufficient, therefore, that the matter of the loadstone should contain many similar particles; they must likewise be disposed in such a manner as to form canals continued from one extremity to the other, in order to grant an uninterrupted transmission to the magnetic matter.

Iron and steel, then, apparently contain such particles in great abundance; these are not, however, originally disposed in the manner I have been describing, but are scattered over the whole mass, and this disposition is all they want to become real magnets. In that case, they still retain all their other qualities, and are not distinguishable from other masses of iron and steel, except that now they have besides the properties of the loadstone; a knife and a needle answer the same purposes, whether they have or want the magnetic virtue. The change which takes place in the interior, from the arrangement of the particles in the order which magnetism requires, is not externally perceptible; and the iron or steel which has acquired the magnetic force is denominated an *artificial magnet*, to distinguish it from the natural, which resembles a stone, though the magnetic properties are the same in both. You will have a curiosity, no doubt, to be informed in what manner iron and steel may be brought to receive the magnetic force, and so become artificial magnets. Nothing can be more simple; and the vicinity of a loadstone is capable of rendering iron somewhat magnetic: it is the magnetic vortex which produces this effect, even though the iron and loadstone should not come into contact.

However hard iron may appear, the particles which contain the magnetic pores formerly represented are very pliant in substance, and the smallest force is sufficient to change their situation. The magnetic matter of the vortex, entering into the iron, will then easily dispose the first magnetic pores which it meets following its own directions—those at least whose situation is not very different; and having run through them, it will act in the same manner on the adjacent pores, till it has forced a passage quite through the iron, and thereby formed some magnetic canals. The figure of the iron contributes greatly to facilitate this change; a lengthened figure, and placed in the same direction with the vortex, is most adapted to it, as the magnetic matter, in passing through the whole length, disposes there a great many particles in their just situation, in order to form longer magnetic canals; and it is certain, that the more there is the means of forming canals, and the longer they are without interruption, the more rapid will be the motion of the magnetic matter, and the greater the magnetic force.

It has likewise been remarked, that when the iron placed in a magnetic vortex is violently shaken or struck, it acquires a higher degree of magnetism from this, because the minute particles are by such concussion agitated and disengaged, so as to yield more easily to the action of the magnetic matter which penetrates them.

Fig. 119.



Placing accordingly a small bar of iron *a b*, *Fig.* 119, in the vortex of the loadstone A B, so that its direction may nearly agree with that of the current *d e f* of the magnetic matter, it will with ease pass through the bar, and form in it magnetic canals, especially if at the same time the bar is shaken or struck to facilitate the transmission. It is likewise observable, that the magnetic matter which enters at the pole A of the loadstone, and escapes at the pole B, will enter the bar at the extremity *a*, and escape at the extremity *b*, so that the extremity *a* will become the pole of the same name A, and *b* the same with B. Then taking this bar *a b* out of the magnetic vortex, it will be an artificial magnet, though very feeble, which will supply its own vortex, and preserve its magnetic power, as long as its magnetic canals shall not be interrupted. This will take place so much the more easily that the pores of iron are pliant; thus the same circumstance which assists the production of magnetism contributes likewise to its destruction. A natural magnet is not so easily enfeebled, because the pores are much closer, and more considerable efforts are requisite to derange them. I shall go more largely into the detail afterward.

I here propose to explain the manner of most naturally rendering iron magnetic; though the force which it thence acquires is very small, it will assist us in comprehending this remarkable and almost universal phenomenon. It has been observed, that tongs and other implements of iron which are usually placed in a vertical position, as well as bars of iron fixed perpendicularly on steeples, acquire in time a very sensible magnetic force. It has likewise been perceived that a bar of iron, hammered in a vertical position or heated red-hot, on being plunged into cold water in the same position, becomes somewhat magnetic, without the application of any loadstone.

In order to account for this phenomenon, you have only to recollect that the earth itself is a loadstone, and consequently encompassed with a magnetic vortex, of which the declination and inclination of the magnetic needle everywhere show the true direction. If then a bar of iron remain long in that position, there is no reason to be surprised should it become magnetic. We have likewise seen that the inclina-

tion of the magnetic needle is at Berlin 72 degrees; and as it is nearly the same all over Europe, this inclination differs only 18 degrees from the vertical position; the vertical position, accordingly, differs but little from the direction of the magnetic vortex: a bar of iron, long kept in that position, will be at last penetrated with the magnetic vortex, and must consequently acquire a magnetic force.

In other countries, where the inclination is imperceptible, which is the case near the equator, it is not the vertical, but rather the horizontal position, which renders bars of iron magnetic; for their position must correspond to the magnetic inclination, if you would have them acquire a magnetic force. I speak here only of iron; steel is too hard for the purpose, and means more efficacious must be employed to communicate the magnetic virtue to it.*

10*th November*, 1761.

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* Captain Scoresby has lately discovered a method of making artificial magnets, solely from the process of hammering soft steel. He found that a bar of soft steel, 6½ inches long, ¼ of an inch in diameter, and weighing 392 grains, when hammered in a vertical direction, on a surface of metal not ferruginous, acquired, after seventeen blows, a lifting power of 6½ grains. When a similar bar was hammered, with its lower end resting on the top of a small poker, it lifted a nail of 88 grains weight, after twenty-two blows. When the poker had been previously hammered in a vertical position, a single blow gave the bar a lifting power of 20 grains; and in one instance ten blows produced a lifting power of 188 grains. When a single blow was struck upon the bar when held with the other end up, its magnetism was almost entirely destroyed.

These curious results have a most important practical application. Captain Scoresby has shown how we may by this process convert the blade of a penknife, the limb of a pair of scissors, or even a nail, into a compass-needle, which will traverse with great facility when suspended by a hair or a slender thread. By this means the shipwrecked mariner may guide himself in his boat as accurately as if he had been able to use his compass. For further information on this subject see the *Edinburgh Transactions*, vol. ix. p. 243; *Philosophical Transactions*, 1822, p. 241; and *Edinburgh Philosophical Journal*, vol. ix. p. 41.—*Ed.*

LETTER LXV.

Action of Loadstones on Iron. Phenomena observable on placing Pieces of Iron near a Loadstone.

Though the whole earth may be considered as a vast loadstone, and as encompassed with a magnetic vortex which everywhere directs the magnetic needle, its magnetic power is, however, very feeble, and much less than that of a very small loadstone: this appears very strange, considering the enormous magnitude of the earth.

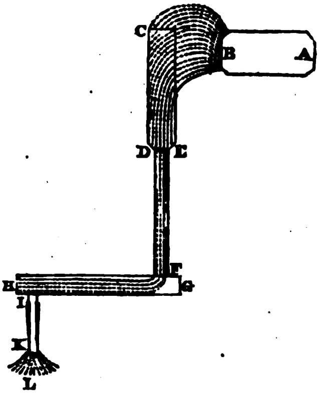
It arises undoubtedly from our very remote distance from the real magnetic poles of the earth, which, from every appearance, are buried at a great depth below the surface: now, however powerful a loadstone may be, its force is considerable only when it is very near; and as it removes, that force gradually diminishes, and at length disappears. For this reason the magnetic force acquired in time by masses of iron suitably placed in the earth's vortex is very small, and indeed hardly perceptible, unless it is very soft, and of a figure adapted to the production of a vortex, as has been already remarked.

This effect is much more considerable near a loadstone of moderate size: small masses of iron speedily acquire from it a very perceptible magnetic force—they are likewise attracted towards the loadstone; whereas this effect is imperceptible in the earth's vortex, and consists only in directing magnetic needles, without either attracting them or increasing their weight.

A mass of iron plunged into the vortex of a loadstone likewise presents very curious phenomena, which well deserve a particular explanation. Not only is this mass at first attracted towards the loadstone, but it too attracts other pieces of iron. Let

A B, *Fig.* 120, be a natural magnet, in the vicinity of which, at the pole B, is placed the mass of iron C D, and it will be found that this last is capable of supporting a bar of iron E F. Apply again to this, at F, an iron ruler G H, in any position whatever, say horizontal, supporting it at H, and it will be found that the ruler is not only attracted by the bar F, but likewise capable of supporting at H needles as I K, and that these needles again act on filings of iron L, and attract them.

Fig. 120.



The magnetic force may thus be propagated to very considerable distances, and even made to change its direction, by the different position of these pieces of iron, though it gradually diminishes. You are perfectly sensible, that the more powerful the loadstone A B is of itself, and the nearer to it the first mass C D, the more considerable likewise is the effect. The late *Mr. de Maupertuis* had a large load-

stone so powerful that at the distance even of several feet, the mass of iron C D continued to exert a very considerable force.

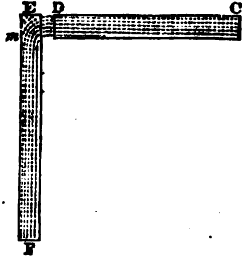
In order to explain these phenomena, you have only to consider that the magnetic matter which escapes rapidly at the pole of the loadstone B enters into the mass of iron, and disposes the pores of it to form magnetic canals, which it afterward freely pervades. In like manner, on entering into the bar, it will there too form magnetic canals—and so on. And as the magnetic matter, on issuing from one body, enters into another, these two bodies must undergo a mutual attraction, for the same reason, as I have before proved, that two loadstones, which present their friendly poles to each other, must be attracted; and as often as we observe an attraction between two pieces of iron, we may with certainty conclude that the magnetic matter which issues from the one is entering into the other, from the continual motion with which it penetrates these bodies. It is thus that, in the preceding disposition of the bars of iron, the magnetic matter in its motion pervades all of them; and this is the only reason of their mutual attraction.

The same phenomena still present themselves on turning the other pole A of the loadstone, by which the magnetic matter enters, towards the mass of iron. The motion in this case becomes retrograde, and preserves the same course; for the magnetic matter contained in the mass of iron will then escape from it, to pass rapidly into the loadstone, and in making its escape will employ the same efforts to arrange the pores in the mass suitably to the current, as if it were rapidly entering into the iron. To this end, therefore, the iron must be sufficiently soft and these pores pliant to obey the efforts of the magnetic matter. A difficulty will no doubt here occur to you; it will be asked, How do you account for the change of direction of the magnetic matter on entering into another bar of iron; and why is that direction regulated according to the length of the bars, as its course is represented in the figure? This is a very important article in the theory of magnetism, and it proves how much the figure of the pieces of iron contributes to the production of the magnetic phenomena.

To elucidate this, it must be recollected that this subtile matter moves with the utmost ease in the magnetic pores, where it is separated from the ether; and that it encounters very considerable obstacles when it escapes from them, with all its velocity, to re-enter into the ether and the air.

Let us suppose that the magnetic matter, after having pervaded the bar C D, *Fig.* 121, enters into the iron ruler E F, placed perpendicularly. It would certainly, on its first admission, preserve the same direction, in order to escape at *m*, unless it found an easier road in which to continue its motion: but meeting at *m* the greatest obstruction, it at first changes its direction, though in a very small degree, towards F, where finding pores adapted to the continuation of its motion, it will deviate more and more from its first direction, and travel through the ruler E F in all its length; and, as if the magnetic matter were loath to leave the iron, it endeavours to continue its motion there as long as possible, availing itself of the length of the ruler; but if the ruler were very short, it would undoubtedly escape at *m*. But the length of the ruler presenting it a space to run through, it follows the direction E F, till it is under the necessity of escaping at F, where all the

Fig. 121.



magnetic canals, formed according to the same direction, no longer permit the subtile magnetic matter to change its direction and return along the ruler; these canals being not only filled with succeeding matter, but, from their very nature, incapable of receiving motion in an opposite direction.

14*th November*, 1761.

LETTER LXVI.

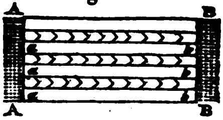
Arming of Loadstones.

You have just seen how iron may receive the magnetic current of a loadstone, convey it to considerable distances, and even change its direction. To unite a loadstone, therefore, to pieces of iron, is much the same with increasing its size, as the iron acquires the same nature with respect to the magnetic matter; and it being further possible by such means to change the direction of the magnetic current, as the poles are the places where this matter enters the loadstone and escapes, we are enabled to conduct the poles at pleasure.

On this principle is founded the arming, or mounting, of loadstones—a subject well worthy of your attention, as loadstones are thus brought to a higher degree of strength.

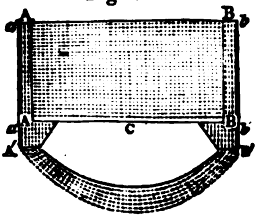
Loadstones, on being taken from the mine, are usually reduced to the figure of a parallelopiped, or rectangular parallelogram, with thickness as A A, B B, *Fig.* 122, of which the surface A A is the pole by which the magnetic matter enters, and B B that by which it escapes. It is filled, then, the whole length A B with canals *a b*, which the magnetic matter, impelled by the elastic power of the ether, freely

Fig. 122.



pervades with the utmost rapidity, and without any mixture of that fluid. Let us now see in what manner such a loadstone is usually armed.

Fig. 123.



To each surface, A A and B B, *Fig*. 123, the two poles of the loadstone, are applied plates of iron *a a* and *b b*, terminating below in the knobs A' and B' called the feet; this is what we denominate the *armour* of the loadstone, and when this is done, the loadstone is said to be armed. In this state, the magnetic matter which would have escaped at the surface B B passes into the iron plate *b b*, where the difficulty of flying off into the air, in its own direction, obliges it to take a different one, and to flow along the plate *b b* into the foot B', and there it is under the necessity of escaping, as it no longer finds iron to assist the continuation of its motion. The same thing takes place on the other side; the subtile matter will be there conducted through the foot A', from which it will pass into the plate *a a*, changing its direction to enter into the loadstone, and to fly through its magnetic canals. For the subtile matter contained in the plate enters first into the loadstone; it is followed by that which is the foot A', replaced in its turn by the external magnetic matter, which, being there impelled by the elasticity of the ether, penetrates the foot A' and the plate *a a* with a rapidity whose vehemence is capable of arranging the poles, and of forming magnetic canals.

Hence it is evident that the motion must be the same on both sides, with this difference, that the magnetic matter will enter by the foot A', and escape by the foot B', so that in these two feet we now find the poles of the armed loadstone; and as

the poles formerly diffused over the surfaces A A and B B are now collected on the basis of the feet A′ and B′, it is naturally to be supposed that the magnetic force must be considerably greater in these new poles.

In this state, accordingly, the vortex will be more easily formed. The matter escaping from the foot B′ will, with the utmost facility, return to the foot A′, passing through C; and the rest of the body of the loadstone will not be encompassed by any vortex, unless perhaps a small quantity of magnetic matter should escape from the plate *b b*, from its not being able to change the direction so suddenly; and unless a small quantity should find admission by the plate *a a*, which in that case might produce a feeble vortex, whereby the subtile matter would be immediately conducted from the plate *b b* to *a a*; however, if the armour be well fitted, this second vortex will be almost imperceptible, and consequently the current between the feet is so much the stronger.

The principal direction for arming loadstones is carefully to polish both surfaces of the loadstone A A and B B, as well as the plates of iron, so that on applying them to the loadstone, they may exactly touch it in every point, the subtile matter passing easily from the loadstone to the iron, when unobstructed by any intervening matter; but if there be a vacuum, or a body of air, between the leadstone and the plates, the magnetic matter will lose almost all its motion, its current will be interrupted, and rendered incapable of forcing its passage through the iron, by forming magnetic canals in it.

The softest and most ductile iron is to be preferred for the construction of such armour, because its pores are pliant, and easily arrange themselves in conformity to the current of the magnetic matter. Iron of this description, accordingly, appears well adapted to the production of a sudden change in the direction of the current: the magnetic matter, too, seems to affect a progress in that direction as long as possible, and does not quit it till the continuance of its motion through that medium is no longer practicable: it prefers making a circuit to a premature departure—a thing that does not take place in the loadstone itself, in which the magnetic canals are already formed, nor in steel, whose pores do not so easily yield to the efforts of a magnetic current. But when these canals are once formed in steel, they are not so easily deranged, and much longer retain their magnetic force; whereas soft iron, whatever force it may have exerted during its junction with a loadstone, loses it almost entirely on being disjoined.

Experience must be consulted as to the other circumstances of arming loadstones. Respecting the plates, it has been found that a thickness either too great or too small is injurious; but for the most part, the best adapted plates are very thin, which would appear strange, did we not know that the magnetic matter is much more subtile than ether, and that consequently the thinnest plate is sufficient to receive a very great quantity of it.

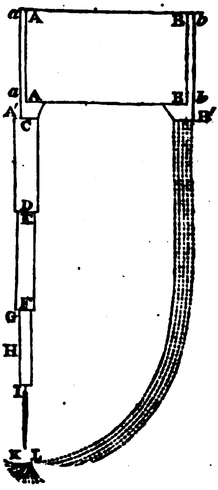
17*th November*, 1761.

LETTER LXVII.

Action and Force of armed Loadstones.

At the feet of its armour, then, a loadstone exerts its greatest force, because there its poles are collected; and each foot is capable of supporting a weight of iron, greater or less in proportion to the excellence of the loadstone.

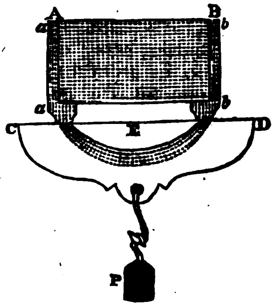
Fig. 124.



Thus a loadstone A A, B B, *Fig.* 124, armed with plates of iron *a a* and *b b*, terminating in the feet A' and B', will support by the foot A' not only the iron ruler C D, but this last will support another of smaller size E F, this again another still smaller G H, which will in its turn support a needle I K, which, finally, will attract filings of iron L; because the magnetic matter runs through all these pieces to enter at the pole A'; or if it were the other pole by which the magnetic matter issues from the loadstone, it would in like manner run through the pieces C D, E F, G H, I K. Now, as often as the matter is transmitted from one piece to another, an attraction between the two pieces is observable; or rather they are impelled towards each other by the surrounding ether, because the current of the magnetic matter between them diminishes the pressure of that fluid.

When one of the poles of the loadstone is thus loaded, its vortex undergoes a very remarkable change of direction; for as, without this weight, the magnetic matter which issues from the pole B′, directing around its course, would flow towards the other pole A′; and as now the entrance into this pole is sufficiently supplied by the pieces which it supports, the matter issuing from the pole B′ must take quite a different road, which will at length conduct it to the last piece I K. A portion of it will undoubtedly be likewise conveyed towards the last but one G H, and towards those which precede it; as those which follow, being smaller, do not supply in sufficient abundance those which go before: but the vortex will always be extended to the last piece. By these means, if the pieces are well proportioned to each other in length and thickness, the loadstone is capable of supporting much more than if it were loaded with a single piece, in which the figure likewise enters principally into consideration. But in order to make it sustain the greatest possible weight, we must contrive to unite the force of both poles.

Fig. 127.



For this purpose, there is applied to the two poles A and B, *Fig.* 127, a piece of soft iron C D, touching the base of the feet in all points, and whose figure is such, that the magnetic matter which issues from B shall find it in the most commodious passage to re-enter at the other extremity A.—Such a piece of iron is called the supporter of the loadstone; and as the magnetic matter enters into it on issuing

from the loadstone at B, and enters into the other pole A, on issuing from the supporter, the iron will be attracted at both poles at once, and consequently adhere to them with great force. In order to know how much power the loadstone exerts, there is affixed to the supporter, at the middle E, a weight P, which is increased till the loadstone is no longer capable of sustaining it; and then that weight is said to counterbalance the magnetic power of the loadstone: this is what you are to understand when told that such a loadstone carries ten pounds weight, such another thirty, and so on. Mahomet's coffin, they pretend, is supported by the force of a loadstone—a thing by no means impossible, as artificial magnets have already been constructed which carry more than 100 pounds weight.

A loadstone armed with its supporter loses nothing of the magnetic matter, which performs its complete vortex within the loadstone and the iron, so that none of it escapes into the air. Since then magnetism exerts its power only in so far as the matter escapes from one body to enter into another, a loadstone whose vortex is shut up should nowhere exert the magnetic power; nevertheless, when it is touched on the plate at *a* with the point of a needle, a very powerful attraction is perceptible, because the magnetic matter, being obliged suddenly to change its direction, in order to enter into the canals of the loadstone, finds a more commodious passage by running through the needle, which will consequently be attracted to the plate *a a*. But by that very thing the vortex will be deranged inwardly; it will not flow so copiously into the feet; and if you were to apply several needles to the plate, or iron rulers still more powerful, the current towards the feet would be entirely diverted, and the force which attracts the supporter would altogether disappear, so that it would drop off without effort. Hence it is evident that the feet lose their magnetic power in proportion as the loadstone exercises its force in other places; and thus we are enabled to account for a variety of very surprising phenomena, which without the theory, would be absolutely inexplicable.

This is the proper place for introducing the experiment which demonstrates, that after having applied its supporter to an armed loadstone, you may go on from day to day increasing the weight which it is able to sustain, till it at length shall exceed the double of what it carried at first. It is necessary to show, therefore, how the magnetic force may in time be increased in the feet of the armour. The case above described, of the derangement of the vortex, assures us, that at the moment when the supporter is applied, the current of the magnetic matter is still abundantly irregular, that a considerable part of it is still escaping by the plate *b b*, and that it will require time to form magnetic canals in the iron: it is likewise probable that, when the current shall have become more free, new canals may be formed in the loadstone itself, considering that it contains, besides these fixed canals, moveable poles, as iron does. But on violently separating the supporter from the loadstone, the current being disturbed, and these new canals in a great measure destroyed, the force is suddenly rendered as small as at the beginning; and some time must intervene before these canals, with the vortex, can recover their preceding state. I once constructed an artificial magnet, which at first could support only ten pounds weight; and after some time I was surprised to find that it could support more than thirty. It is remarked, chiefly in artificial magnets, that time alone strengthens them considerably; but that this increase of force lasts only till the supporter is separated from it.

21st November, 1761.

LETTER LXVIII.

The Method of communicating to Steel the Magnetic Force, and of magnetizing Needles for the Compass: the Simple Touch, its Defects; Means of remedying these.

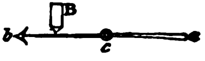
Having explained the nature of magnets in general, an article as curious as interesting still remains; namely, the manner of communicating to iron, but especially to steel, the magnetic power, and even the highest degree possible of that power.

You have seen that, by placing iron in the vortex of a loadstone, it acquires a magnetic force, but which almost totally disappears as soon as it is removed out of the vortex; and that the vortex of the earth alone is capable, in time, of impressing a slight magnetic power upon iron; now, steel being harder than iron, and almost entirely insensible to this action of the magnetic vortex, more powerful operations must be employed to magnetize it; but then it retains the magnetic force much longer.

For this purpose we must have recourse to touching, and even to friction. I begin, therefore, with explaining the method formerly employed for magnetizing the needles of compasses; the whole operation consisted in rubbing them at the pole with a good loadstone, whether naked or armed.

The needle *a b c*, *Fig.* 125, was laid on a table; the pole B of the loadstone was drawn over it, from *b* towards *a*, and, being arrived at the extremity *a*, the loadstone was raised aloft, and brought back through the air to *b*; this operation was repeated several times together, particular care being taken that the other pole of the loadstone should not come

Fig. 125.



near the needle, as this would have disturbed the whole process. Having several times drawn the pole B of the loadstone over the needle, from *b* to *a*, the needle had become magnetic, and the extremity *b* of the same name with that of the loadstone with which it had been rubbed. In order to render the extremity *b* the north pole, it would have been necessary to rub with the pole of this name in the loadstone, proceeding from *b* to *a*; but in rubbing with the south pole, the progress must be from *a* to *b*.

This method of rubbing, or touching, is denominated *the simple touch*, because the operation is performed by touching with one pole only; but it is extremely defective, and communicates but very little power to the needle, let the loadstone be ever so excellent; accordingly, it does not succeed when the steel is carried to the highest degree of hardness, though this be the state best adapted to the retention of magnetism. You will yourself readily discern the defects of this method by *the simple touch*.

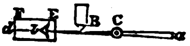
Let us suppose that B is the pole of the loadstone from which the magnetic matter issues, as the effect of the two poles is so similar that it is impossible to perceive the slightest difference; having rested the pole on the extremity *b* of the needle, the magnetic matter enters into it with all the rapidity with which it moves in the loadstone, incomparably greater than that of the vortex which is in the external air. But what will become of this matter in the needle? It cannot get out at the extremity *b*, it will therefore make an effort to force its way through the needle towards *a*, and the pole B, moving in the same direction, will assist this effort; but as soon as the pole B shall arrive at *a*, the difficulty of escaping at the extremity *a* will occasion a contrary effort, by which the magnetic matter will be impelled from *a* towards *b*; and before the first effect is entirely destroyed this last cannot take place. Afterward, when the

pole B is again brought back to the extremity *b*, this last effect is again destroyed, but without producing, however, a current in the contrary direction from *b* towards *a*; and consequently, when the pole B shall have got beyond *c* in its progress towards *a*, it will more easily produce a current from *a* to *b*, especially if you press more hard on the half *c a*: hence it is clear, that the needle can have acquired only a small degree of the magnetic power.

Some, accordingly, rub only the half *c a* (*Fig.* 125, p. 238), proceeding from *c* to *a*, and others touch only the extremity *a* of the needle with the pole B of the loadstone, and with nearly the same success. But it is evident that the magnetic matter which enters by the extremity *a* only is incapable of acting with sufficient vigour on the pores of the needle, for arranging them conformably to the laws of magnetism; and that the force impressed by this method must be extremely small, if any thing, when the steel is very much hardened.

It appears to me, then, that these defects of *the simple touch* might be remedied in the following manner; of the success of which I entertain no doubt, though I have not yet tried it; but am confirmed in my opinion by experiments which I have made.

Fig. 126.



I would case the extremity *b* of the needle, *Fig.* 126, in a ruler of soft iron E F; and I should think it proper to make that ruler very thin, and as straight as possible; but the extremity must be exactly applied in all points, and even fitted to a groove perfectly adjusted for its reception. On resting the pole B of the loadstone upon the extremity *b* of the needle, the magnetic matter which enters into it, meeting scarcely any difficulty in its progress through the iron ruler, will at once pursue its course in the direction *b d*; and in proportion as the pole advances towards *a*, the magnetic matter, in order to continue this course, has only to arrange the pores on which it immediately acts; and having reached *a*, all these pores, or at least by far the greater number of them, will be already disposed conformably to that direction. When you afterward recommence the friction at the extremity *b*, nothing is destroyed; but you continue to perfect the current of the magnetic matter, following the same direction *b d*, by likewise arranging the pores which resisted the first operation; and thus the magnetic canals in the needle will always become more perfect. A few strokes of the pole B will be sufficient for the purpose, provided the loadstone is not too weak; and I have no doubt that the best tempered steel, that is, rendered as hard as possible, would yield to this method of operating; an unspeakable advantage in the construction of compasses, as it has been found that ordinary needles frequently lose, by a slight accident, all their magnetic power; by which ships at sea would be exposed to the greatest dangers, if they had not others in reserve. But when needles are made of well tempered steel, accidents of this kind are not so much to be apprehended; for if a greater force is requisite to render them magnetic, in return they preserve the power more tenaciously.

24*th November*, 1761.

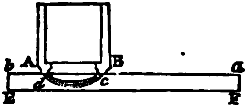
LETTER LXIX.

On the Double Touch. Means of preserving the Magnetic matter in Magnetized Bars.

Instead of this method of magnetizing iron or steel by *the simple touch*, by rubbing with one pole only of the loadstone, we now employ *the double touch*, in which we rub with both poles at once,

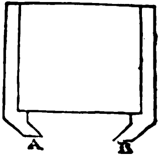
which is easily done by means of an armed loadstone.

Fig. 130.



Let E F, *Fig.* 130, be a bar of iron or steel, which you wish to render magnetic. Having fixed it steadily on a table, you press upon it the two feet A and B of an armed loadstone. In this state, you will easily see that the magnetic matter which issues from the loadstone by the foot B must penetrate into the bar, and would diffuse itself in all directions, did not the foot A, on its side, attract the magnetic matter contained in the pores of the bar. This evacuation therefore at *d* will determine the matter which enters by the pole B to take its course from *c* towards *d*, provided the poles A and B are not too remote from each other. Then the magnetic current will force its way in the bar, in order to pass from the pole B to the pole A, disposing its pores to form magnetic canals; and it is very easy to discover whether this is taking place; you have only to observe if the loadstone is powerfully attracted to the bar, which never fails if the bar is of soft iron, as the magnetic matter easily penetrates it. But if the bar is of steel, the attraction is frequently very small—a proof that the magnetic matter is incapable of opening for itself a passage from *c* to *d*; hence it is to be concluded that the loadstone is too feeble, or that the distance between its two poles is too great: in this case it would be necessary to employ a loadstone more powerful, or whose feet are nearer; or finally, the armour of the loadstone ought to be changed into the form reoresented in *Fig.* 129.

Fig. 129.



But the following is a method of remedying this inconvenience.

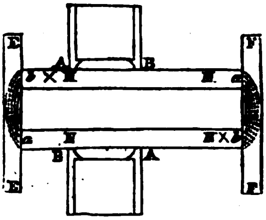
Having fixed the bar as in *c d*, *Fig*. 130 (p. 242), the loadstone must be several times drawn backward and forward over it, from one extremity to the other, without taking it off till you perceive that the attraction no longer increases; for it is undoubtedly certain that attraction is increased in proportion to the increase of the magnetic force. The bar E F will be magnetized by this operation in such a manner that the extremity E, towards which the pole A was turned, will be the friendly pole of A, and consequently of the same name with the other pole B. Again, on removing the loadstone, as magnetic canals are formed the whole length of the bar, the magnetic matter diffused through the air will force a passage through these canals, and will make the bar a real magnet. It will enter by the extremity *a*, and escape by the extremity *b*, from whence a part, at least, will return to *a*, and will form a vortex such as the nature of the bar permits.

I take this occasion to remark, that the formation of a vortex is absolutely necessary to the increase of magnetism: for if all the magnetic matter which goes out at the extremity *b* were to fly off, and be entirely dispersed, without returning to *a*, the air would not supply a sufficient quantity to the other extremity *a*, which must occasion a diminution of the magnetic force. But if a considerable part of that which escapes at the extremity *b* returns to *a*, the air is abundantly able to supply the remainder, and perhaps still more, if the magnetic canals of the bar are capable of receiving it; the bar will therefore in that case acquire a much greater magnetic force.

This consideration leads me to explain how it is possible to keep up the magnetic matter in magnetized bars. The object being to prevent the magnetic matter which pervades them from dispersing in the

air, these bars are always disposed in *pairs* of exactly the same size. They are placed on a table, in a parallel situation, so that the friendly poles, or those of different names, should be turned to the same side as in *Fig*. 131, where M M and N N represent the two bars, whose friendly poles *a*, *b*, *b*, *a*, are turned the same way. To prevent mistake, a mark × is made on each bar, at the extremity where the north pole is, and to both ends is applied a piece of soft iron E E and F F, for receiving the magnetic current. In this manner, the whole magnetic matter which pervades the bar M M, and which issues at the extremity *b*, passes into the piece of iron E E, where it easily makes its way, to enter at the extremity *a* of the other bar N N, from which it will escape at the extremity *b*, into the other piece of iron F F, which reconveys it into the first bar M M by the extremity *a*. Thus the magnetic matter will continue to circulate, and no part of it escape; and even in case there should not be at first a sufficient quantity to supply the vortex, the air will supply the deficiency, and the vortex will preserve all its force in the two bars.

Fig. 131.

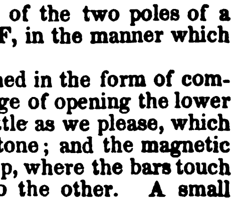


This disposition of the two bars may likewise be employed for magnetizing both of them at once. The two poles of a loadstone must be drawn over the two bars, passing from the one to the other by the pieces of iron; and the circuit must be several times performed, carefully observing that the two poles of the loadstone A and B be turned as the figure directs.

This method of magnetizing two bars at once must be much more efficacious than the preceding, as from the very first circuit performed by the loadstone, the magnetic matter will begin to flow through the two bars by means of the two pieces of iron. Afterward, by repeated circuitous applications of the loadstone to the bars, a greater quantity of pores will be arranged in them conformably to magnetism, and more magnetic canals will be opened, by which the vortex will be more and more strengthened, without undergoing any diminution. If the bars are thick, it would be proper to turn and rub them in the same manner on the other surfaces, in order that the magnetic action may penetrate them thoroughly.

Having obtained these magnetic bars M M, N N, *Fig.* 132, they may be employed in place of the natural loadstone, for magnetizing others. They are joined together at the top, so that the two friendly poles *a b* may touch each other; and the other two poles below, *b* and *a*, are separated as far as it is thought proper. Then we rub with the two under extremities, which supply the place of the two poles of a loadstone, two other bars E F, in the manner which I have above explained.

Fig. 132.



As these two bars are joined in the form of compasses, we have the advantage of opening the lower extremities as much or as little as we please, which cannot be done with a loadstone; and the magnetic current will easily pass at top, where the bars touch each other, from the one to the other. A small piece of soft iron P might likewise be applied there, the better to keep up the current; and in this manner you may easily and speedily magnetize as many double bars as you please.

28th November, 1761.

LETTER LXX.

The Method of communicating to Bars of Steel a very great magnetic Force, by means of other Bars which have it in a very inferior degree.

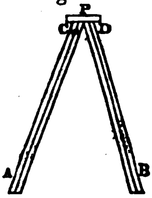
THOUGH this method of magnetizing by *the double touch* be preferable to the preceding, the magnetic power, however, cannot be carried beyond a certain degree. Whether we employ a natural loadstone or two magnetic bars for rubbing other bars, these last will never acquire so much force as the first; it being impossible that the effect should be greater than the cause.

If the bars with which we rub have little force, those which are rubbed will have still less: the reason is evident; for as bars destitute of magnetic force never could produce it in others, so a moderate degree of force is incapable of producing one greater than itself, at least by the method which I have been describing.

But this rule is not to be taken in the strict interpretation of the words, as if it were literally impossible to produce a greater magnetic force by the assistance of a smaller. I am going to point out a method by which the magnetic power may be increased almost as far as you please, beginning with the smallest degree possible. This is a late discovery, which merits so much the more attention that it throws much light on a very difficult subject—the nature of magnetism.

Supposing that I am possessed of a very feeble loadstone, or, for want of a natural magnet, of bars of iron rendered somewhat magnetic merely by the vortex of the earth, as I explained it in a preceding Letter, I then provide myself with eight bars of steel, very small, and not hardened, in order the more easily to receive the small degree of magnetic power which the feeble loadstone, or slightly magnetized bars, are capable of communicating, by rubbing each pair or couple in the manner I formerly described. Having then eight bars magnetic, but in a very small degree, I take two pair, which I join together in the manner represented in *Fig.* 133.

Fig. 133.



By uniting the two bars by the poles of the same name, I form but one of double the thickness, and with which I form the compass A C and B D; the better to keep up the magnetic current, a piece of soft iron P may be applied at the top C D. The legs of the compass may be separated as far as is judged proper, and I rub with them, one after the other, the remaining bars, which will thereby acquire more power than they had before, because the powers of the first are now united. I have now only to join these two pair newly rubbed in the same manner, and by rubbing with them, one after the other, the two pair first employed, and the power of these will be considerably increased. I afterward join these two pair together, and go on rubbing others, in order to augment their magnetic force, and still two pair with two pair alternately; and by repeating this operation, the magnetic power may be carried to such a degree as to become insusceptible of further increase, even by continuing the operation. When we have more than four pair of such bars, instead of two pair, three may be joined together for the purpose of rubbing others; they will thereby be sooner carried to the highest degree possible.

The greatest obstacles are therefore surmounted; and by means of such bars, joined together by two or more pairs, we may rub others of steel properly hardened, and which may be either of the same size, or still greater than the first, to which the greatest power of which they are susceptible may be thus communicated.

Beginning with small bars such as I have de

scribed, these operations may be successively applied to bars of an enormous size, and made of the hardest steel, which is less liable to lose the magnetic power. Only it is to be observed, that for the purpose of rubbing large bars, several pairs ought to be joined together, whose united weight should be at least double that of the large one. But it would always be better to proceed by degrees, and to rub each species of bars with bars not much smaller than themselves, or it may be sufficient to join at most two pair: for when we are obliged to join more than two pair, the extremities with which the friction is performed will extend too far, and the magnetic matter which passes that way will itself prevent its being directed conformably to the direction of the bar that is rubbed; and the rather that it enters the bar perpendicularly, whereas it necessarily should take a horizontal direction.

In order to facilitate this change of direction, it is proper that the magnetic matter should be led to it in a small space, and in a direction already approaching to that which it ought to take within the bar which we are going to rub. The following method, I think, might be effectual for this purpose.

Fig. 134.

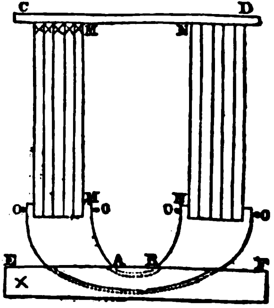


Fig. 134 represents five pair of bars M M, N N, joined together, but not in the form of a compass. There is at top a bar of soft iron C D, to keep up the vortex; below, I do not rub immediately with the extremities of the bars, but I case these extremities on each side in a foot of soft iron, fastening them to it with screws marked O. Each foot is bent at A and B, so that the direction of the magnetic matter, which freely pervades these feet, already has a considerable approximation to the horizontal; so that in the bar to be rubbed E F it has no need greatly to change its direction. I have no doubt, that by means of these feet the bar E F will receive a much greater magnetic power than if we rubbed immediately by the extremities of the bars, the depth of whose vertical direction naturally opposes the formation of horizontal magnetic canals in the bar E F. It is likewise possible, in practising this method, to contract or extend the distance of the feet A and B at pleasure.

I must further observe, that when these bars lose in time their magnetic power, it is easily restored by the same operation.

1st December, 1761.

LETTER LXXI.

Construction of Artificial Magnets in the Form of a Horse-shoe.

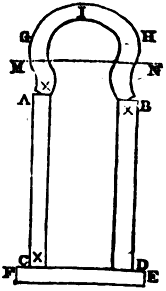
Whoever wishes to make experiments on the properties of the loadstone ought to be provided with a great number of magnetic bars, from a very small up to a very large size. Each may be considered as a particular magnet, having its two poles, the one north and the other south.

You must have considered it as extremely remarkable, that by the interposition of the magnetic power, the feeblest which can be supplied by a wretched natural loadstone, or by a pair of tongs in the chimney corner, which have acquired by length of time a small portion of magnetism, we should be enabled to increase that power to such a degree as to communicate to the largest bars of steel the highest degree of magnetic force of which they are susceptible. It would be needless to add, that by this method we are enabled to construct the best magnetic needles, not only much larger than the common, but made of a steel hardened to the highest degree, which renders them more durable. I have only a few words to add on the construction of artificial magnets, which have usually the form of a horse-shoe, as you must no doubt have seen.

These artificial magnets answer the same purposes on every occasion as the natural ones, with this advantage in their favour, that we can have them much more powerful, by giving them a sufficient magnitude. They are made of well tempered steel, and the figure of a horse-shoe seems the most proper for keeping up the vortex. When the mechanic has finished his work, we communicate to it the greatest degree of magnetic power of which it is susceptible, by means of the magnetic bars, of which I have given a description. It is evident, that the greater this magnet is, the larger must be the bars we employ: and this is the reason why we should be provided with bars of all sizes.

In order to magnetize a horse-shoe HIG, *Fig.* 135, which ought to be of steel well tempered, we place on the table a pair of magnetic bars A C and B D, with their supporters of soft iron applied on both sides, but of which the figure represents only one E F, the other having been removed to make way gradually for the application of the feet of the horse-shoe, as you see. In this state, the magnetic matter which pervades the bars will make strong efforts to pass through the horse-shoe, the poles of the bars being adapted magnetically to those of the horse-shoe; but considering the hardness of tempered steel, it will not be suffi-

Fig. 135.

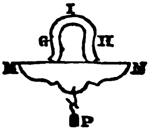


cient to arrange the pores, and open for itself a passage. The same means, therefore, must be employed to this effect which were prescribed for the magnetizing of bars. We take a compass formed of another pair of magnetic bars, and rub them in the same manner over the horse-shoe; magnetic canals will thereby be opened, and the subtile matter of the bars, by pervading it, will form the vortex of that fluid. Particular care must be taken, in this operation, that the legs of the compass, in passing over the horse-shoe, do not touch the extremities A and B of the bars; for this would disturb the current of the magnetic matter, which would pass immediately from the bars into the legs of the compass; or the vortices of the bars and of the compass would mutually derange each other.

The horse-shoe will thereby acquire very great power, being pervaded by an impetuous magnetic current. All that remains to be done is to detach the bars without deranging the current. If they are separated violently, the magnetic vortex will be destroyed, and the artificial magnet will retain very little power.

The canals being kept up no longer than the magnetic matter pervades them, it must be concluded that the particles which form these canals are in a forced state, and that this state subsists only while the vortex acts; and that as soon as it ceases, these particles, by their elasticity, will deviate from their forced situation, and the magnetic canals will be interrupted and destroyed. This we clearly see in the case of soft iron, whose pores are quickly arranged on the approach of a magnetic vortex, but retain scarcely any magnetic power when removed out of the vortex. This proves that the pores of iron are moveable, but endowed with an elasticity which changes their situation as soon as force ceases. It requires length of time to fix certain pores in the position impressed on them by the magnetic force, which takes place chiefly in bars of iron long exposed to the vortex of the earth. The pores of steel are much less flexible, and better support the state into which they have been forced: they are however liable to some derangement, as soon as force ceases to act on them; but this derangement is less in proportion to the hardness of the steel. For this reason artificial magnets ought to be made of the hardest steel: were they to be made of iron, they would immediately acquire, on being applied to magnetic bars, a very great degree of power; but the moment you detach them, all that power would disappear. Great precaution must therefore be employed in separating from the bars magnets composed of well tempered steel. For this purpose, before the separation, you press the supporter, which is of very soft iron, in the direction of the line M N, *Fig.* 136, taking particular care not to touch the bars with it, for this would mar the whole process, and oblige you to repeat the operation. On the application of the supporter, a considerable portion of the magnetic matter which is circulating in the magnet G H I will make its way through the supporter, and form a separate vortex, which will continue after the magnet is detached from the bars.

Fig. 136.



Afterward, you press the supporter slowly forward over the legs of the magnet to the extremities, as represented in the figure, and in this state permit it to rest for some time, that the vortex may be allowed to settle. The supporter is likewise furnished with a weight P, which may be increased every day; it being always understood that the supporter is to be so perfectly adjusted to the feet of the magnet as to touch them in all points.*

5th December, 1761.

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* The laws of magnetism have been ably investigated during the last ten years, and many highly interesting facts have been added to our pre-

LETTER LXXII.

On Dioptrics; Instruments which that Science supplies: of Telescopes and Microscopes. Different Figures given to Glasses or Lenses.

The wonders of dioptrics will now, I think, furnish a subject worthy of your attention. This science provides us with two kinds of instruments composed of glass, which serve to extend our sphere of vision, by discovering objects which would escape the naked eye.

There are two cases in which the eye needs assist-

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vious knowledge of this department of physics. It has been conclusively shown that the magnetic fluid is, if not identical with the electric, so intimately associated with it as to appear to be the same thing modified only by some peculiarities of motion or mode of excitement. The important discovery was made by Professor Œrsted, of Copenhagen, that when a current of Voltaic electricity is passing along the surface of a bar of copper, tin, lead, iron, or other metal, the bar possesses magnetic properties, and will cause an immediate deviation of a magnetic needle suspended near it; that it has its north and south poles, the situation of which depends on the direction of the electric current; that these poles are immediately changed by changing the direction of the current; that the bar of metal, which, thus conducting the electric fluid, will attract iron filings, and in short convert iron or steel into magnets, in the same manner as a natural loadstone or artificial magnet. So effectual is this mode of producing artificial magnets that Professor Henry, of Albany, relates an experiment performed by himself and Dr. Ten Eyck, in which a bar of iron bent in the form of a horse-shoe, and weighing 59½ lbs. avoirdupois, being surrounded with a coil of copper wire 728 feet long, and the ends of the wire connected with galvanic batteries containing 4 7-9ths square feet of surface, the iron became instantly so powerfully magnetized by the revolution of the electric current around it as to sustain a weight attached to its armature or lifter of 2000 lbs.—(*Vide American Journal of Science*, vol. xx. p. 201.)

The discovery of Œrsted and the numerous and successful investigations of the nature of the connexion between electricity and magnetism have laid the foundation of a new branch of physics, called *electro-magnetism*. A spark, similar to the electric spark, has been obtained from an iron magnet alone, unconnected with an electric or galvanic battery.—(*American Journal of Science*, vol. xxii. p. 410.)

Of the identity of the two agents there can therefore be no longer a doubt. The theory of our author of course is untenable.—*Am. Ed.*

ance: the first is, when objects are too distant to admit of our seeing them distinctly; such are the heavenly bodies, respecting which the most important discoveries have been made by means of dioptrical instruments. You will please to recollect what I have said concerning the satellites of Jupiter, which assist us in the discovery of the longitude; they are visible only with the aid of good telescopes; and those of Saturn require telescopes of a still better construction.

There are, besides, on the surface of the earth objects very distant, which it is impossible for us to see, and to examine in detail, without the assistance of telescopes, which represent them to us in the same manner as if they were near. These dioptrical glasses or instruments for viewing distant bodies are denominated *telescopes*.

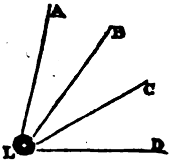
The other case in which the eye needs assistance is when the object, though sufficiently near, is too small to admit of a distinct examination of its parts. If we wished, for example, to discover all the parts of the leg of a fly, or of any insect still smaller,—if we were disposed to examine the minuter particles of the human body, such as the smallest fibres of the muscles, or of the nerves, it would be impossible to succeed without the help of certain instruments called *microscopes*, which represent small objects in the same manner as if they were a hundred or a thousand times greater.

Here, then, are two kinds of instruments, *telescopes* and *microscopes*, furnished by dioptrics for assisting the weakness of our sight. A few ages only have elapsed since these instruments were invented; and from the era of that invention must be dated the most important discoveries in astronomy by means of the telescope, and in physics by the microscope.

These wonderful effects are produced merely by the figure given to bits of glass, and the happy combination of two or more glasses, which we denominate *lenses*. Dioptrics is the science that unfolds the principles on which such instruments are constructed, and the uses to which they are applied; and you will please to recollect that it turns chiefly on the direction which rays of light take on passing through transparent media of a different quality; on passing, for example, from air into glass or water, and reciprocally, from glass or water into air.

As long as the rays are propagated through the same medium, as for example air, they preserve the same direction, in the straight lines L A, L B, L C, L D, *Fig.* 128, drawn from the luminous point L, whence these rays issue; and when they anywhere meet an eye they enter into it, and there paint an image of the object from which they proceeded. In this case the vision is denominated simple, or natural; and represents to us the objects as they really are. The science which explains to us the principles of this vision is termed *optics*.

Fig. 128.



But when the rays, before they enter into the eye, are reflected on a finely polished surface, such as a mirror, the vision is no longer natural; as in this case we see the objects differently, and in a different place, from what they really are. The science which explains the phenomena presented to us by this vision from reflected rays is termed *catoptrics*. It, too, supplies us with instruments calculated to extend the sphere of our vision; and you are acquainted with such sorts of instruments, which, by means of one or two mirrors, render us the same services as those constructed with lenses. These are what we properly denominate *telescopes*; but in order to distinguish them from the common perspectives, which are composed only of glasses, it would be better to

call them catoptric or reflecting telescopes. This mode of expression would at least be more accurate; for the word telescope was in use before the discovery of reflecting instruments, and then meant the same thing with perspective.

I propose at present to confine myself entirely to dioptrical instruments, of which we have two sorts, telescopes and microscopes. In the construction of both we employ glasses formed after different manners, the various sorts of which I am going to explain. They are principally three, according to the figure given to the surface of the glass.

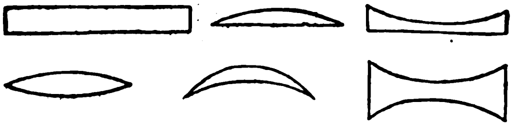
The first is the *plane*, when the surface of a glass is plane on both sides, as that of a common mirror If you were to take, for example, a piece of looking-glass, and to separate from it the quicksilver which adheres to its farther surface, you would have a glass both of whose surfaces are *plane*, and of the same thickness throughout.

The second is the *convex*; a glass of this denomination is more raised in the middle than towards the edge.

The third is the *concave*; such a glass is hollow towards the middle, and rises towards the edge.

Of these three different figures which may be given to the surface of a glass, are produced the six species of glasses represented in *Fig*. 133.

Fig. 133.



I. The *plane* glass has both its surfaces plane.

II. The *plano-convex* glass has one surface plane and the other convex.

III. The *plano-concave* has one surface plane and the other concave.

IV. The *convexo-convex*, or *double convex*, has both surfaces convex.

V. The *convexo-concave*, or *meniscus*, has one surface convex and the other concave.

VI. Finally, the *concavo-concave*, or *double concave*, has both surfaces concave.

It is proper to remark, that the figure represents the section of these glasses or lenses.

8th December, 1761.

LETTER LXXIII.

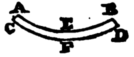
Difference of Lenses with respect to the Curve of their Surfaces. Distribution of Lenses into three Classes.

From what I have said respecting the convex and concave surfaces of lenses, you will easily comprehend that their form may be varied without end, according as the convexity and concavity are greater or less. There is only one species of plane surfaces, because a surface can be plane in one manner only; but a convex surface may be considered as making part of a sphere, and according as the radius or diameter of that sphere is greater or less, the convexity will differ; and as we represent lenses on paper by segments of a circle, according as these circles are greater or less, the form of lenses must be infinite, with respect both to the convexity and concavity of their surfaces.

As to the manner of forming and polishing glasses, all possible care is taken to render their figure exactly circular or spherical; for this purpose we employ basins of metal formed by the turning machine, on a spherical surface, both inwardly and outwardly.

Let A E B D F C, *Fig.* 137, be the form of such a basin, which shall have two surfaces, A E B and C F D, each of which may have its separate radius; when a piece of glass is rubbed on the concave side of the basin A E B, it will become convex; but if it is rubbed on the convex side C F D, it will become concave. Sand, or coarse emery, is at first used in rubbing the glass on the basin, till it has acquired the proper form; and after that a fine species of emery, or pumice-stone, to give it the last polish.

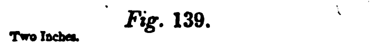
Fig. 137.



In order to know the real figure of the surfaces of a lens, you have only to measure the radius of the surface of the basin on which that lens was formed; for the true measure of the convexity and concavity of surfaces, is the radius of the circle or sphere which corresponds to them, and of which they make a part.

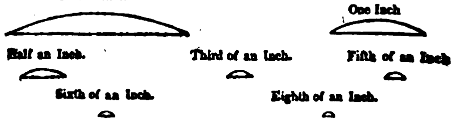
Thus, when it is said that the radius of the convex surface A E B, *Fig.* 138, is three inches, the meaning is, that A E B is an arch of a circle described with a radius of three inches, the other surface A B being plane.

Fig. 138.



That I may convey a still clearer idea of the difference of convexities, when their radii are greater or less, I shall here present you with several figures of different convexity, *Fig.* 139.

Fig. 139.



From this you see, that the smaller the radius is the greater is the curve of the surface, or the greater its difference from the plane; on the contrary, the greater the radius is, the more the surface approaches to a plane, or the arch of the circle to a straight line. If the radius were made still greater, the curve would at length become hardly perceptible. You scarcely perceive it in the arch M N, *Fig.* 138, the radius of which is six inches, or half a foot; and if the radius were still extended to ten or a hundred times the magnitude, the curve would become altogether imperceptible to the eye.

But this is by no means the case as to dioptrics; and I shall afterward demonstrate, that though the radius were a hundred or a thousand feet, and the curve of the lens absolutely imperceptible, the effect would nevertheless be abundantly apparent. The radius must indeed be inconceivably great to produce a surface perfectly plane: from which you may conclude, that a plane surface might be considered as a convex surface whose radius is infinitely great, or as a concave of a radius infinitely great. Here it is that convexity and concavity are confounded, so that the plane surface is the medium which separates convexity from concavity. But the smaller the radii are, the greater and more perceptible do the convexities and concavities become; and hence we say, reciprocally, that a convexity or concavity is greater in proportion as its radius, which is the measure of it, is smaller.

However great in other respects may be the variety we meet with in lenses or glasses, according as their surfaces are plane, convex, or concave, and this in an infinity of different manners; nevertheless, with respect to the effect resulting from them in dioptrics, they may be reduced to the three following classes:—

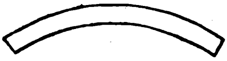
The first comprehends glasses which are everywhere of an equal thickness; whether their two sur-

faces be plane and parallel to each other, *Fig*. 140, or the one convex and the other concave, but con-

Fig. 140.

centric, or described round the same centre, *Fig*. 141, so that the thickness shall remain everywhere the same. It is to be remarked respecting glasses

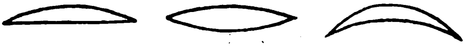
Fig. 141.



of this class, that they produce no change in the appearance of the objects which we view through them; the objects appear exactly the same as if nothing interposed; accordingly, they are of no manner of use in dioptrics. This is not because the rays which enter into these glasses undergo no refraction, but because the refraction at the entrance is perfectly straightened on going off, so that the rays, after having passed through the glass, resume the same direction which they had pursued before they reached it. Glasses, therefore, of the other two classes, on account of the effect which they produce, constitute the principal object of dioptrics.

The second class of lenses contains those which are thicker at the middle than at the edge, *Fig*. 142.

Fig. 142.

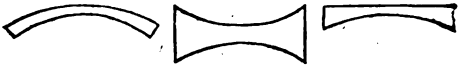


Their effect is the same, as long as the excess of the thickness of the middle over that of the edge has the same relation to the magnitude of the lens. All lenses of this class are commonly denominated *con-*

vex, as convexity predominates, though otherwise one of their surfaces may be plane, and even concave.

The third class contains all those lenses which are thicker at the edge than in the middle, *Fig*. 143,

Fig. 143.



which all produce a similar effect, depending on the excess of thickness towards the edge over that in the middle. As concavity prevails in all such lenses, they are simply denominated *concave*. They must be carefully distinguished from those of the second class, which are the convex.

Lenses of these two last classes are to be the subject of my following Letters, in which I shall endeavour to explain their effects in dioptrics.

12*th December*, 1761.

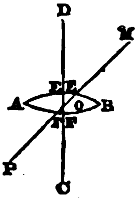
LETTER LXXIV.

Effect of Convex Lenses.

In order to explain the effect produced by both convex and concave lenses in the appearance of objects, two cases must be distinguished; the one when the object is very far distant from the lens, and the other when it is nearer.

But before I enter on the explanation of this, I must say a few words on what is called the axis of the lens. As the two surfaces are represented by segments of a circle, you have only to draw a straight line through the centres of the two circles; this line is named the axis of the lens. In *Fig*. 144, the centre of the arch A E B being at C, and that of the arch A F B at D, the straight line C D is denominated the axis of the lens A B; and it is easy to see that this axis passes through the middle of it. The same thing would apply if the surfaces of the lens were concave. But if one is plane, the axis will be perpendicular to it, passing through the centre of the other surface.

Fig. 144.



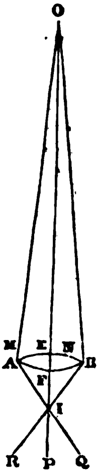
Hence it is obvious that the axis passes through the two surfaces perpendicularly, and that accordingly a ray of light coming in the direction of the axis will suffer no refraction, because rays passing from one medium into another are not broken or refracted, except when they do not enter in a perpendicular direction.

It may likewise be proved that all other rays passing through the middle of the lens O undergo no refraction, or rather that they again become parallel to themselves.

It must be considered, in order to comprehend the reason of this, that at the points E and F the two surfaces of the lens are parallel to each other, for the angle M E B which the ray M E makes with the arch of the circle E B, or its tangent at E, is perfectly equal to the angle P F A, which this same ray produced, or F P makes with the arch of the circle A F, or its tangent at F: you recollect that two such angles are denominated alternate, and that it is demonstrated, when the alternate angles are equal, that the straight lines are parallel to each other; consequently, the two tangents at E and at F will be parallel, and it will be the same thing as if the ray M E F P passed through a lens whose two surfaces were parallel to each other. Now we have already seen that rays do not change their direction in passing through such a lens.

Having made these remarks, let us now consider a convex lens A B, *Fig*. 145, whose axis is the straight line O E F P; and let us suppose that there is in this line, at a great distance from the lens, an object or luminous point O, which diffuses rays in all directions: some of these will pass through our lens A B, such as O M, O E, and O N; of which that in the middle, O E, will undergo no refraction, but will continue its direction through the lens in the same produced straight line F I P. The other two rays, O M and O N, in passing through the lens towards the edge, will be refracted both at entering and departing, so that they will somewhere meet the axis, as at I, and afterward continue their progress in the direction I Q and I R. It might likewise be demonstrated that all the rays which fall between M and N will be refracted, so as to meet with the axis in the same point I. Therefore, the rays which, had no lens interposed, would have pursued their rectilineal direction O M and O N, will, after the refraction, pursue other directions, as if they had taken their departure from the point I: and if there were an eye somewhere at P, it would be affected just as if the luminous point were actually at I, though there be no reality in this. You have only to suppose for a moment, that there is at I a real object, which diffusing its rays, would be equally seen by an eye placed at P, as it now sees the object at O by means of the rays refracted by the lens, because there is at I an image of the object O, and the lens A B there represents the object O, or transports it nearly to I. The point O is therefore no longer the object of vision, but rather its image, represented at I; for this is now its immediate object.

Fig 145.



This lens, then, produces a very considerable change: an object very remote O is suddenly transported to I, from which the eye must undoubtedly receive a very different impression from what it would do if, withdrawing the lens, it were to view the object O immediately. Let O be considered as a star, the point O being supposed extremely distant, the lens will represent at I the image of that star, but an image which it is impossible to touch, and which has no reality, as nothing exists at I, unless it be that the rays proceeding from the point O are collected there by the refraction of the lens. Neither is it to be imagined that the star would appear to us in the same manner as if it really existed at I. How could a body many thousands of times bigger than the earth exist at a point I? Our senses would be very differently struck by it. We must carefully remark, then, that an image only is represented at I, like that of a star represented in the bottom of the eye, or that which we see in a mirror, the effect of which has nothing to surprise us.

15th December, 1761.

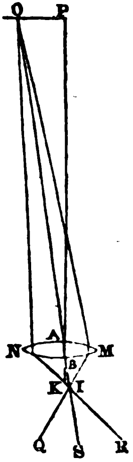
LETTER LXXV.

The same Subject: Distance of the Focus of Convex Lenses.

I MEAN to employ this Letter in explaining the effect produced by convex lenses, that is, such as are thicker at the middle than at the edge. The whole consists in determining the change which rays undergo in their progress, on passing through such a glass. In order to place this subject in its clearest light, two cases must be carefully distinguished; the one when the object is very distant from the lens, and the other when it is at no great distance. I begin with considering the first case, that is, when the object is extremely remote from the lens.

In *Fig*. 146, M N is the convex lens, and the straight line O A B I S its axis, passing perpendicularly through the middle. I remark, by-the-way, that this property of the axis of every lens, that of passing perpendicularly through its middle, conveys the justest idea of it that we are capable of forming. Let us now conceive that on this axis there is somewhere at O an object O P, which I here represent as a straight line, whatever figure it may really have; and as every point of this object emits its rays in all directions, we confine our attention to those which fall on the lens.

Fig. 146.



My remarks shall be at present further limited to the rays issuing from the point O, situated in the very axis of the lens. The figure represents three of these rays, O A, O M, and O N, the first of which, O A passing through the middle of the lens, undergoes no change of direction, but proceeds, after having passed through the lens, in the same straight line B I S, that is, in the axis of the lens; but the other two rays, O M and O N, undergo a refraction both on entering into the glass and leaving it, by which they are turned aside from their first direction, so as to meet somewhere at I with the axis, from which they will proceed in their new direction, in the straight lines M I Q and N I R; so that afterward, when they shall meet an eye, they will produce in it the same effect as if the point O existed at I, as they preserve the same direction. For this reason, the convex lens is said to transport the object O to I; but in order to distinguish this point I from the real point O, the former is called the image of the latter, which in its turn is denominated the object.

This point I is very remarkable, and when the object O is extremely distant, the image of it is likewise denominated the focus of the lens, of which I shall explain the reason. If the sun be the object at O, the rays which fall on the lens are all collected at I; and being endowed with the quality of heating, it is natural that the concourse of so many rays at I should produce a degree of heat capable of setting on fire any combustible matter that may be placed there. Now, the place where so much heat is collected we call the *focus;* the reason of this denomination with respect to convex lenses is evident. Hence, too, a convex lens is denominated a *burning-glass*, the effects of which you are undoubtedly well acquainted with. I only remark, that this property of collecting the rays of the sun in a certain point, called their focus, is common to all convex lenses: they likewise collect the rays of the moon, of the stars, and of all very distant bodies; though their force is too small to produce any heat, we nevertheless employ the same term, focus: the focus of a glass, accordingly, is nothing else but the spot where the image of very distant objects is represented; to which this condition must still be added, that the object ought to be situated in the very axis of the lens; for if it be out of the axis, its image will likewise be represented out of the axis. I shall have occasion to speak of this afterward.

It may be proper still further to subjoin the following remarks respecting the focus:—

1. As the point O, or the object, is infinitely distant, the rays O M, O A, and O N may be considered as parallel to each other; and, for the same reason, parallel to the axis of the lens.

2. The focus I, therefore, is the point behind the glass where the rays parallel to the axis which

fall on the lens are collected by the refraction of the lens.

3. The focus of a lens, and the spot where the image of an object, infinitely distant, and situated in the axis of the lens, is represented, are the same thing.

4. The distance of the point I behind the lens, that is, the length of the line B I, is called the distance of the focus of the lens. Some authors call it the *focal distance*, or *focal length.*

5. Every convex lens has its particular distance of focus—one greater, another less—which is easily ascertained by exposing the lens to the sun, and observing where the rays meet.

6. Lenses formed by arches of small circles, have their focuses very near behind them; but those whose surfaces are arches of great circles have more distant focuses.

7. It is of importance to know the focal distance of every convex lens employed in dioptrics; and it is sufficient to know the focus in order to form a judgment of all the effects to be expected from it, whether in the construction of telescopes or microscopes.

8. If we employ lenses equally convex on both sides, so that each surface shall correspond to the same circle, then the radius of that circle gives nearly the focal distance of that lens; thus, to make a burning-glass which shall burn at the distance of a foot, you have only to form the two surfaces arches of a circle whose radius is one foot.

9. But when the lens is plano-convex, its focal distance is nearly equal to the diameter of the circle which corresponds to the convex surface.

Acquaintance with these terms will facilitate the knowledge of what I have further to advance on this subject.

19th December, 1761.

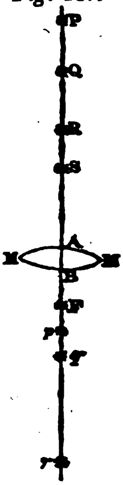
LETTER LXXVI.

Distance of the Image of Objects.

Having remarked that an object infinitely distant is represented by a convex lens in the very focus, provided the object be in the axis of the lens, I proceed to nearer objects, but always situated in the axis of the glass; and I observe, first, that the nearer the object approaches to the lens the farther the image retires.

Let us accordingly suppose that F, *Fig.* 147, is the focus of the lens M M, so that when an object is infinitely distant before the glass, or at the top of the figure, the image shall be represented at F; on bringing the object nearer to the glass, and placing it successively at P, Q, R, the image will be represented at the points *p*, *q*, *r*, more distant from the lens than the focus: in other words, if A P is the distance of the object, B *p* will be the distance of the image; and if A Q is the distance of the object, B *q* will be that of the image; and the distance B *r* of the image will correspond to the distance A R of the object.

Fig. 147.



There is a rule by which it is easy to calculate the distance of the image behind the lens for every distance of the object before it, but I will not tire you with a dry exposition of this rule; it will be sufficient to remark, in general, that the more the distance of the object before the glass is diminished, the more is the distance of the image behind it increased. I shall to this subjoin the instance of a convex lens whose focal distance is six inches, or of a lens so formed that if the distance of the object is infinitely great, the distance of the image behind the lens shall be precisely six inches; now, on bringing the object nearer to the lens, the image will retire, according to the gradations marked in the following table:

Distance of the Object.	*Distance of the Image.*
Infinity.	6
42	7
24	8
18	9
15	10
12	12
10	15
9	18
8	24
7	42
6	Infinity.

Thus, the object being 42 inches distant from the lens, the image will fall at the distance of 7 inches, that is, one inch beyond the focus. If the object is at the distance of 24 inches, the image will be removed to the distance of 8 inches from the lens, that is, two inches beyond the focus; and so of the rest.

Though these numbers are applicable only to a lens whose focal distance is 6 inches, some general consequences may, however, be deduced from them.

1. If the distance of the object is infinitely great, the image falls exactly in the focus.

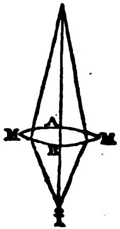
2. If the distance of the object is double the distance of the focus, the distance of the image will likewise be double the distance of the focus; in other words, the object and the image will be equally distant from the lens. In the example above exhibited,

the distance of the object being 12 inches, that of the image is likewise 12 inches.

3. When the object is brought so near the lens that the distance is precisely equal to that of the focus, say 6 inches, as in the preceding example, then the image retires to an infinite distance behind the lens.

4. It is likewise observable in general, that the distance of the object and that of the image reciprocally correspond; or if you put the object in the place of the image, it will fall in the place of the object.

Fig. 148.



5. If, therefore, the lens M M, *Fig*. 148, collects at I the rays which issue from the point O, the same lens will likewise collect at O rays issuing from the point I.

6. It is the consequence of a great principle in dioptrics, in virtue of which it may be maintained that whatever are the refractions which rays have undergone in passing through several refracting media, they may always return in the same direction.

This truth is of much importance in the knowledge of lenses: thus, when I know, for example, that a lens has represented, at the distance of 8 inches, the image of an object 24 inches distant, I may confidently infer, that if the object were 8 inches distant, the same lens would represent its image at the distance of 24 inches.

It is further essential to remark, that when the distance of the object is equal to that of the focus, the image will suddenly retire to an infinite distance; which perfectly harmonizes with the relation existing between the object and the image.

You will no doubt be curious to know in what place the image will be represented when the object is brought still nearer to the lens, so that its distance shall become less than that of the focus. This question is the more embarrassing, that the answer must be, the distance of the image will in this case be greater than infinity, since the nearer the object approaches the lens the farther does the image retire. But the image being already infinitely distant, how is it possible that distance should be increased? The question might undoubtedly puzzle philosophers, but is of easy solution to the mathematician. The image will pass from an infinite distance to the other side of the lens, and consequently will be on the same side with the object. However strange this answer may appear, it is confirmed, not only by reasoning, but by experience, so that it is impossible to doubt of its solidity; to increase beyond infinity is the same thing with passing to the other side: this is unquestionably a real paradox.

22*d December*, 1761.

LETTER LXXVII.

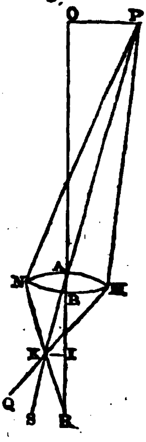
Magnitude of Images.

You can no longer doubt that every convex lens must represent somewhere the image of an object presented to it; and that in every case the place of the image varies as much according to the distance of the object as according to the focal distance of the lens: but a very important article remains yet to be explained—I mean the magnitude of the image.

When such a lens represents to us the image of the sun, of the moon, or of a star, at the distance of a foot, you are abundantly sensible that these images must be incomparably smaller than the objects themselves. A star being much greater than the whole earth, how is it possible that an image of such magnitude should be represented to us at the distance of a foot? But the star appearing to us only as a point, the image represented by the lens likewise resembles a point, and consequently is infinitely smaller than the object itself.

There are, then, in every representation made by lenses, two things to be considered; the one respects the place where the image is represented, and the other the real magnitude of the image, which may be very different from that of the object. The first being sufficiently elucidated, I proceed to furnish you with a very simple rule, by which you will be enabled in every case to determine what must be the magnitude of the image represented by the lens.

Fig. 149.



Let O P, *Fig.* 149, be any object whatever situated on the axis of the convex lens M N; we must first look for the place of the image, which is at I, so that the point I shall be the representation of the extremity O of the object, as the rays issuing from the point O are there collected by the refraction of the lens. Let us now see in what place will be represented the other extremity P of the object; for this purpose let us consider the rays P M, P A, P N, which, issuing from the point P, fall on the lens. I observe that the ray P A, which passes through the middle of the lens, does not change its direction, but continues its progress in the straight line A K S; it will be therefore somewhere in this line, at K, that the other rays P M and P N will meet: in other words, the point K will be the image of the other extremity P of the object, the point I being that of the extremity O;

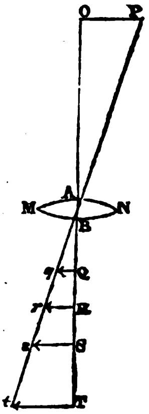
hence it is easy to conclude that I K will be the image of the object O P represented by the lens.

In order, then, to determine the magnitude of this image, having found the place I, you have only to draw from the extremity P of the object, through A, the middle of the lens, the straight line P A K S, and to raise from I the line I K perpendicular to the axis, and this line I K will be the image in question; it is evident from this that the image is reversed, so that if the line O R were horizontal, and the object O P a man, the image would have the head K undermost, and the feet I uppermost.

On this I subjoin the following remarks:

1. The nearer the image is to the lens, the smaller it is; and the more remote it is, the greater its magnitude. Thus, O P, *Fig.* 150, being the object placed on the axis before the lens M N, if the image fell at Q, it would be smaller than if it fell at R, S, or T. For, as the straight line P A *t*, drawn from the summit of the object P, through the middle of the lens, always terminates the image, at whatever distance it may be, it is evident that among the lines Q *q*, R *r*, S *s*, T *t*, the first Q *q* is the smallest, and that the others increase in proportion as they remove from the lens.

Fig. 150.



2. There is one case in which the image is precisely equal to the object: it is when the distance of the image is equal to that of the object; and this takes place, as I have already remarked, when the distance of the object A O is double that of the focus of the lens; the image will then be T *t*, so that the distance B *t* is equal to A O. You have only then to consider the two triangles O A P and T A *t*.

which having the opposite angles at the point A, as well as the sides A O and A T, equal each to each, as likewise the angles at O and T, which are both right angles; these two triangles will be every way equal, and consequently the side T *t*, which is the image, will be equal to the side O P, which is the object.

3. If the image were twice farther from the lens than the object, it would be double the object; and in general, as many times as the image is farther from the lens than the object, so many times will it be greater than the object. For the nearer you bring the object to the glass, the farther the image retires, and consequently the greater it becomes.

4. The contrary takes place when the image is nearer the lens than the object; it is then as many times smaller than the object as it is nearer the lens than the object is. If, then, the distance of the image were one thousand times less than that of the object, it would likewise be one thousand times smaller.

5. Let us apply this to burning-glasses, which, being exposed to the sun, represent its image in the focus, or rather represent the focus, that is, the luminous and brilliant circle, which burns, and which is nothing else but the image of the sun represented by the lens. You will no longer be surprised, then, at the smallness of the image, notwithstanding the prodigious magnitude of the sun, it being as many times smaller in the focus than the real sun, as the distance of the sun from the lens is greater than that of the image.

6. Hence likewise it is evident, that the greater is the distance of the focus of a burning-glass, the more brilliant also is the circle in the focus, that is, the greater will be the image of the sun; and the diameter of the focus is always about one hundred times smaller than the distance of the focus from the lens.

I shall afterward explain the different uses which may be made of convex lenses; they are all sufficiently curious to merit attention.

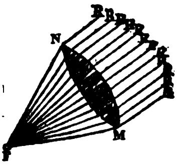
26th December, 1761.

LETTER LXXVIII.

Burning-glasses.

The first use of convex lenses is their employment as burning-glasses, the effect of which must appear altogether astonishing, even to those who already have some acquaintance with natural philosophy. In fact, who could believe that the image of the sun simply should be capable of exciting such a prodigious degree of heat? But your surprise will cease, if you please to pay some attention to the following reflections:—

Fig. 151.



1. Let M N, *Fig.* 151, be a burning-glass, which receives on its surface the rays of the sun R, R, R, refracted in such a manner as to present at F a small luminous circle, which is the image of the sun, and so much smaller as it is nearer to the glass.

2. All the rays of the sun, which fall on the surface of the glass are collected in the small space of the focus F; their effect, accordingly, must in that space be as many times greater as the surface of the glass exceeds the magnitude of the focus, or of the sun's image. We say that the rays, which were dispersed over the whole surface of the glass, are concentrated in the small space F.

3. The rays of the sun having a certain degree of heat, they exert their power in a very sensible manner at the focus; it is possible even to calculate how

many times the heat at the focus must exceed the natural heat of the sun's rays: we have only to observe how many times the surface of the glass is greater than the focus.

4. If the glass were not greater than the focus, the heat would not be stronger at the focus than anywhere else; hence we must conclude, that in order to the production of a strong heat by a burning-glass, it is not sufficient that it should be convex, or that it should represent the image of the sun; it must besides have a surface which several times exceeds the magnitude of the focus, which is smaller in proportion as it is nearer to the glass.

5. France is in possession of the most excellent burning-glass: it is three feet in diameter, and its surface is calculated to be nearly two thousand times greater than the focus, or the image of the sun which it represents.* It must produce, therefore, in the focus, a heat two thousand times greater than that which we feel from the sun. Its effects are accordingly prodigious: wood of every kind is in a moment set on fire; metals are melted in a few minutes; and, in general, the most ardent fire which we are capable of producing is not once to be compared with the vehement heat of this focus.

6. The heat of boiling water is calculated to be about thrice greater than what we feel from the rays of the sun in summer; or, which amounts to the same thing, the heat of boiling water is thrice greater than the natural heat of the blood in the human body. But in order to melt lead, we must have a heat thrice greater than is requisite to make water

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* The lens here alluded to was, we believe, one of *Tschirnhausen's*, that the Duke of Orleans purchased for the Academy of Sciences. A more powerful burning lens, however, was afterward made in England by *Mr. Parker*, which cost above 700*l*. It had 2 feet 8½ inches of clear diameter. Its thickness at the centre was 3¼ inches, and its focal length 6 feet 8 inches in diameter. It was made of flint-glass. This celebrated lens is now at Pekin.—See *Edinburgh Encyclopædia*, article *Burning Instruments*, vol. v. p. 141.—*Ed.*

boil; and to melt copper, a heat still thrice greater is necessary. To melt gold requires a much higher degree of heat. Heat, then, one hundred times greater than that of our blood is capable of melting gold; how far then must a heat two thousand times greater exceed the force of our ordinary fires?

7. But how are these prodigious effects produced by the rays of the sun collected in the focus of a burning-glass? This is a very difficult question, with respect to which philosophers are very much divided. Those who maintain that the rays are an emanation from the sun, darted with the amazing velocity which I formerly described, are not greatly embarrassed for a solution; they have only to say that the matter of the rays, striking bodies with violence, must totally break and destroy their minute particles. But this opinion is no longer admitted in sound philosophy.

8. The other system, which makes the nature of light to consist in the agitation of the ether, appears little adapted to explain these surprising effects of burning-glasses. On carefully examining, however, all the circumstances, we shall soon be convinced of the possibility of this. The natural rays of the sun, as they fall on bodies, excite the minute particles of the surface to a concussion, or motion of vibration, which, in its turn, is capable of exciting new rays; and by these the body in question is rendered visible. And a body is illuminated only so far as these proper particles are put into a motion of vibration so rapid as to be capable of producing new rays in the ether.

9. It is clear, then, that if the natural rays of the sun have sufficient force to agitate the minute particles of bodies, those which are collected in the focus must put the particles which they meet there into an agitation so violent that their mutual adhesion is entirely dissolved, and the body itself completely destroyed, which is the effect of fire. For if

the body is combustible, as wood, the dissolution of these minute particles, joined to the most rapid agitation, makes a considerable part of it to fly off into air in the form of smoke, and the grosser particles remain in the form of ashes. Fusible bodies, as metals, become liquid by the dissolution of their particles, whence we may comprehend how fire acts on bodies; it is only the adhesion of their minutest particles which is attacked, and the particles themselves are thereby afterward put into the most violent agitation. Here, then, is a very striking effect of burning-glasses, which derives its origin from the nature of convex lenses.* There are besides many wonderful effects to be described.

28th December, 1761.

LETTER LXXIX.

The Camera Obscura.

We likewise employ convex lenses in the *camera obscura*, and by means of them all external objects are presented in the darkened room on a white surface, in their natural colours, in such a manner that landscapes and public buildings, or objects in general, are represented in much greater perfection than the power of the pencil is capable of producing. Painters accordingly avail themselves of this method, in order to draw with exactness landscapes and other objects which are viewed at a distance. The camera obscura, then, which is the subject of this Letter, is represented at E F G H, *Fig.* 152, closely shut up

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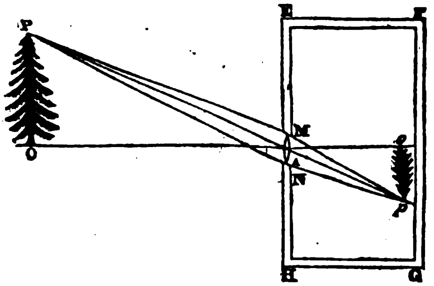
* In the work already quoted, in p. 262, note, I have shown how burning lenses may be constructed of any size, by building them, as it were, of separate zones, each zone consisting of different segments, which are ground and polished separately. By this means the central parts of the burning lens are much less thick than when the lens is of one piece, and the error of the spherical aberration may be in a great measure corrected.—See the *Edinburgh Philosophical Journal*, vol. viii. p. 109.—*Ed.*

Fig. 152.



on all sides, except one little round aperture made in one of the window-shutters, in which is fixed a convex lens, of such a focus as to throw the image of external objects, say the tree O P, exactly on the opposite wall F G, at *o p*. A white and moveable table is likewise employed, which is put in the place of the images represented.

The rays of light, therefore, can be admitted into the chamber only through the aperture M N, in which the lens is fixed, without which total darkness would prevail.

Let us now consider the point P of any object, say the stem of our tree O P. Its rays P M, P A, P N, will fall on the lens M N, and be refracted by it, so as to meet again at the point *p* on the wall, or on a white table* placed there for the purpose. This point *p* will consequently receive no other rays but such as proceed from the point P; and in like manner every other point of the table will receive only the rays which proceed from the corresponding point of the object; and reciprocally, to every point

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* The table should be made of stucco, or plaster of Paris, ground very smoothly, and ought to be concave, that every part of it may be equally distant from the lens.—*Ed.*

of the external object will correspond a point on the table, which receives those rays, and no other. If the lens were to be removed from the aperture M N, the table would be illuminated in quite a different manner; for in that case every point of the object would diffuse its rays over the whole table, so that every point of the table would be illuminated at once by all the external objects, whereas at present it is so by one only, that whose rays it receives: from this you will easily comprehend that the effect must be quite different from what it would be if the rays entered simply by the aperture M N into the chamber.

Let us now examine somewhat more closely wherein this difference consists; and let us first suppose that the point P of the object is green; the point of the table *p* will therefore receive only those green rays of the object P, and these, reuniting on the wall or table, will make a certain impression, which here merits consideration. For this purpose you will please to recollect the following propositions, which I had formerly the honour of explaining to you:—

1. Colours differ from each other in the same manner as musical sounds; each colour is produced by a determinate number of vibrations, which in a given time are excited in the ether. The green colour of our point P is accordingly appropriated to a certain number of vibrations, and would no longer be green were these vibrations more or less rapid. Though we do not know the number of vibrations which produce such or such a colour, we may however be permitted to suppose here that green requires twelve thousand vibrations in a second; and what we affirm of this number, twelve thousand, may likewise be easily understood of the real number, whatever it be.

2. This being laid down, the point *p* on the white table will be struck by a motion of vibration, of which

twelve thousand will be completed in a second. Now, I have remarked that the particles of a white surface are all of such a nature as to receive every sort of agitation, more or less rapid; whereas those of a coloured surface are adapted to receive only that degree of rapidity which corresponds to their colour. And as our table is white, the point *p* in it will be excited to a motion of vibration corresponding to the colour of green; in other words, it will be agitated twelve thousand times in a second.

3. As long as the point *p*, or the particle of the white surface which exists there, is agitated with a similar motion, this will be communicated to the particles of the ether which surround it; and this motion, diffusing itself in all directions, will generate rays of the same nature, that is to say green; just as in music, the sound of a certain note, say C, agitates a string wound up to the same tone, and makes it emit a sound without being touched.

4. The point *p* of the white table will accordingly produce green rays, as if it were died or painted that colour; and what I affirm of the point *p* will equally take place with respect to all the points of the illuminated table, which will produce all the rays, each of the same colour with that of the object whose image it represents. Every point of the table will therefore become visible, under a certain colour, as if it were actually painted that colour.

5. You will perceive, then, on the table, all the colours of the external objects, the rays of which will be admitted into the chamber through the lens; each point in particular will appear of the colour of that point of the object which corresponds to it, and you will see on the table a combination of various colours, disposed in the same order as you see them in the objects themselves; that is to say, a representation, or rather the perfect picture, of all the objects on the outside of the dark chamber which are before the lens N N.

6. All these objects will, however, appear reversed on the table, as you will conclude from what I have said in my foregoing Letters. The under part of the tree O will be represented at *o*, and the summit P at *p*; for, in general, each object must be represented on the white table in the place which is the termination of the straight line drawn from the object P through the middle of the lens A: that which is upward will consequently be represented downward, and that which is to the left will be to the right; in a word, every thing will be reversed in the picture; the representation will nevertheless be more exact and more perfect than the most accurate painter is capable of producing.

7. You will further remark, that this picture will be so much smaller than the objects themselves in proportion as the focus of the lens is shorter. Lenses of a short focus will accordingly give the objects in miniature; and if you would wish to have them magnified, you must employ lenses of a longer focus, or which represent the images at a greater distance.

8. In order to contemplate these representations more at ease, the rays may be intercepted by a mirror, from which they are reflected, so as to represent the whole picture on a horizontal table; and this is of peculiar advantage when we wish to copy the images.*

2d January, 1762.

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* The lens is sometimes ground on the anterior surface of a thick piece of glass, the posterior surface of which is ground flat, and inclined 45° to the axis of the lens. The picture is therefore reflected on a horizontal table, without the use of a mirror, and the image is much more perfect, as the light is totally reflected.—*Ed.*

LETTER LXXX.

Reflections on the Representation in the Camera Obscura.

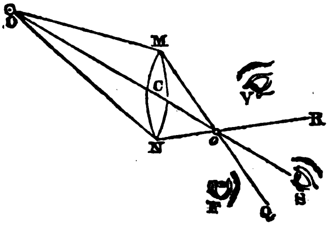
Though you can no longer entertain any doubt respecting the representations made in a dark chamber by means of a convex lens, I hope the following reflections will not appear superfluous, as they serve to place this subject in a clearer light:—

1. The chamber must be completely darkened, for were the light admitted the white table would be visible, and the particles of its surface, already agitated, would be incapable of receiving the impression of the rays which unite to form the images of external objects. Though, however, the chamber were a little illuminated, still something of the representation would appear on the table, but by no means so vivid as if the chamber were entirely dark.

2. We must carefully distinguish the picture represented on the white table from the image which the lens in virtue of its own nature represents, as I have formerly explained. It is very true, that placing the table in the very place where the image of the objects is formed by the lens, this image will be confounded by the picture we perceive on the table; these two things are nevertheless of a nature entirely different: the image is only a spectre or shadow floating in the air, which is visible but in certain places; whereas the representation is a real picture, which every one in the chamber may see, and to which duration alone is wanting.

3. In order the more clearly to elucidate this difference, you have only to consider carefully the nature of the image *o*, *Fig.* 153, represented by the convex lens M N, the object being at O. This image is nothing else but the place in which the rays O M, O C, O N, of the object, after having passed through

Fig. 153.



the lens, meet by refraction, and thence continue their direction as if they proceeded from the point *o*, though they really originated from O, and by no means from *o*.

4. Hence the image is visible only to eyes situated somewhere within the angle R *o* Q, as at S, where an eye will actually receive the rays which come to it from the point *o*. But an eye situated out of this angle, as at F or V, will see nothing at all of it, because no one of the rays collected at *o* is directed towards it: the image at *o*, therefore, differs very essentially from a real object, and is visible only in certain places.

5. But if a white table is placed at *o*, and its surface at this point *o* is really excited to an agitation similar to that which takes place in the object O, this spot *o* of the surface itself generates rays which render it visible everywhere. Here, then, is the difference between the image of an object and its representation made in a camera obscura: the image is visible only in certain places, namely, those through which are transmitted the rays that originally proceed from the object; whereas the picture, or representation formed on the white table, is seen by its own rays, excited by the agitation of the particles of its surface, and consequently visible in every place of the camera obscura.

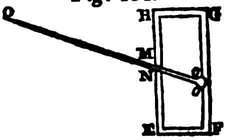
6. It is likewise evident that the white table must absolutely be placed exactly in the place of the image formed by the lens, in order that every point of the table may receive no other rays except such as proceed from a single point of the object; for if other rays were likewise to fall upon it, they would disturb the effect of the former, or render the representation confused.

7. Were the lens to be entirely removed, and free admission given to the rays into the dark chamber, the white table would be illuminated by it, but no picture would be visible. The rays of the different objects would fall on every point of the table, without expressing any one determinate image. The picture, accordingly, which we see in a camera obscura, on a white surface, is the effect of the convex lens fixed in the shutter: this it is which collects anew, in a single point, all the rays that proceed from one point of the object.

8. A very singular phenomenon is here however observable, when the aperture made in the window-shutter of the dark chamber is very small; for though no lens be applied you may nevertheless perceive, on the opposite partition, the images of external objects, and even with their natural colours; but the representation is very faint and confused, and if the aperture is enlarged, this representation entirely disappears. I shall explain this phenomenon.

In *Fig.* 154, M N is the small aperture through which the rays of external objects are admitted into the dark chamber E F G H. The wall F G opposite to the aperture is white, the better to receive the impression of rays of all sorts.

Fig. 154.



Let the point O be an object, of which the rays O M, O N alone, with those which fall between them, can enter into the chamber. These rays will be confined to the small space *o o* of the wall, and will illuminate it. This space *o o* will be so much smaller, or approach the nearer to a point, in proportion as the aperture M N is small: if then this aperture were very small, we should have the effect already described, according to which every point of the white table receives only the rays proceeding from a single point of the object: there would be produced, of consequence, a representation similar to that which is produced by the application of a convex lens to an aperture in the window-shutter. But in the present case, the aperture being of a certain extent, every point O of the object will illuminate a certain small space *o o* on the wall, and agitate it by its rays. The same thing, then, nearly, would take place, as if a painter, instead of making points with a fine pencil, should with a coarse one make spots of a certain magnitude, attending, however, to design and colouring: the representation made on the wall will have a resemblance to this sort of daubing; but it will be clearer in propoftion to the smallness of the aperture by which the rays are admitted.

5th January, 1762.

LETTER LXXXI.

Of the Magic Lantern, and Solar Microscope.

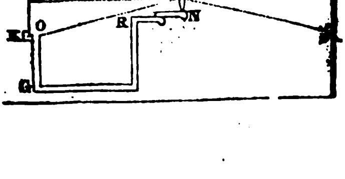
The camera obscura has properly no effect except on very distant objects, but you will easily comprehend that its application may be equally extended to nearer objects. For this purpose, the white table must be removed farther from the lens, conformably to this general rule, that the nearer the object is brought to the convex lens, the farther does the image, where the white table ought to be placed, retire from it; and if the chamber is not of sufficient depth, a different lens, of a shorter focus, must be employed.

You may place, then, out of the chamber, before the aperture to which the convex lens is fitted, any object or picture whatever, and you will see a copy of it on the white table within the dark chamber, greater or smaller than the original, according as the distance of the image is greater or smaller; but it would be more commodious, undoubtedly, if the object could be exposed within the dark chamber, in order to its being moved and changed at pleasure. But here a great difficulty occurs,—the object itself would in this case be darkened, and consequently rendered incapable of producing the effect we wish.

The thing wanted, then, is to illuminate the object as much as possible within the dark chamber, and at the same time to exclude the light. I have found out the means of doing this. You will recollect that I constructed a machine to the effect I am mentioning, which I had the honour of presenting to you six years ago; and now you will easily comprehend the structure, and the principles on which it is founded.

This machine consists of a box very close on all sides, nearly of a figure similar to *Fig*. 164. The

Fig. 164.



farther side of which E G has an opening I K, in which are to be fitted the objects, portraits or other pictures, O P, which you mean to represent; on the other side, directly opposite, is a tube M N Q R, containing a convex lens M N; this tube is moveable, for the purpose of bringing the lens nearer to the object, or of removing it, at pleasure. Then, provided the object O P be well illuminated, the lens will throw somewhere the image of it *o p*, and if you there place a white tablet, you will see upon it a perfect copy of the object, so much the clearer as the object itself is more illuminated.

For this purpose I have contrived in this box two side wings, for the reception of lamps with large wicks, and in each wing is placed a mirror to reflect the light of the lamps on the objects O P; above, at E F, is a chimney, by which the smoke of the lamps passes off. Such is the construction of this machine, within which the object O P may be very strongly illuminated, while the darkness of the chamber suffers no diminution. In order to the proper use of this machine, attention must be paid to the following remarks.

1. On sliding inward the tube M N Q R, that is, bringing the lens M N nearer to the object O P, the image *o p* will retire; the white tablet must therefore be removed backwards, to receive the image at the just distance; the image will thereby be likewise magnified, and you may go on to enlarge it at pleasure, by pressing the lens M N nearer and nearer to the object O P.

2. On removing the lens from the object, the distance of the image will be diminished: the white tablet must in this case be moved nearer to the lens, in order to have a clear and distinct representation; but the image will be reduced.

3. It is obvious that the image will be always reversed; but this inconvenience is easily remedied; you have only to reverse the object O P itself, turn-

ing it upside down, and the image will be represented upright on the white tablet.

4. It is a further general remark, that the more the image is magnified on the white tablet, the less luminous and distinct it will be; but on reducing the image, it is rendered more distinct and brilliant. The reason is plain—the light proceeds wholly from the illumination of the object; the greater that the space is over which it is diffused, the more it must be weakened, and the more contracted it is, the more brilliant.

5. Accordingly, the more you wish to magnify the representation, the more you must strengthen the illumination of the object, by increasing the light of the lamps in the wings of the machine; but for small representations a moderate illumination is sufficient.

The machine which I have been describing is called the *magic lantern*, to distinguish it from the common camera obscura, employed for representing distant objects; its figure, undoubtedly, has procured it the name of lantern, especially as it is designed to contain light; but the epithet *magic* must have been an invention of its first proprietors, who wished to impress the vulgar with the idea of magic or witchcraft. The ordinary magic lanterns, however, are not constructed in this manner, and serve to represent no other objects but figures painted on glass, whereas this machine may be applied to objects of all sorts.

It may even be employed for representing the smallest objects, and for magnifying the representation to a prodigious size, so that the smallest fly shall appear as large as an elephant; but for this purpose the strongest light that lamps can give is far from being sufficient; the machine must be disposed in such a manner that the objects may be illuminated by the rays of the sun, strengthened by a burning-glass; the machine, in this case, changes

its name, and is called the *solar microscope.* I shall have occasion to speak of it more at large in the sequel.

8th January, 1762.

LETTER LXXXII.

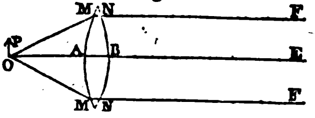
Use and Effect of a simple Convex Lens.

We likewise employ convex lenses for immediately looking through; but in order to explain their different uses, we must go into a closer investigation of their nature.

Having observed the focal distance of such a glass, I have already remarked, that when the object is very remote, its image is represented in the focus itself; but on bringing the object nearer to the lens, the image retires farther and farther from it: so that if the distance of the object be equal to that of the focus of the lens, the image is removed to an infinite distance, and consequently becomes infinitely great.

The reason is, that the rays O M, O M, *Fig.* 155, which come from the point O, are refracted by the lens, so as to become parallel to each other, as N F, N F; and as parallel lines are supposed to proceed forward to infinity, and as the image is always in the place where the rays, issuing from one point of the object, are collected again after the refraction; in the case when the object O A is equal to that of the focus of the lens, the place of the image

Fig. 155.

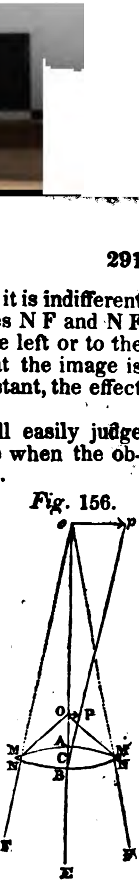


removes to an infinite distance; and as it is indifferent whether we conceive the parallel lines N F and N F to meet at an infinite distance to the left or to the right, it may be said indifferently that the image is to the right or to the left infinitely distant, the effect being always the same.

Having made this remark, you will easily judge what must be the place of the image when the object is brought still nearer to the lens.

Let O P, *Fig.* 156, be the object, and as its distance O A from the convex lens is less than the distance of the focus, the rays O M, O M, which fall upon it from the point O, are too divergent to admit of the possibility of their being rendered parallel to each other by the refractive power of the lens: they will therefore be still divergent after the refraction, as marked by the lines N F, N F, though much less so than before; therefore, if these lines are produced backward, they will meet somewhere at *o*, as you may see in the dotted lines N *o*, N *o*. The rays N F, N F, will of consequence, after having passed through the lens, preserve the same direction as if they had proceeded from the point *o*, though they have not actually passed through that point, as it is only in the lens that they have taken this new direction. An eye which receives these refracted rays N F, N F, will be therefore affected as if they really came from the point *o*, and will imagine that the object of its vision exists at *o*. There will, however, be no image at that point, as in the preceding case. To no purpose would you put a white tablet at *o*; it would present no picture there for want of rays: for this reason we say that there is an imaginary image at *o*, and not a

Fig. 156.



real one—the term *imaginary* being opposed to that of *real.*

Nevertheless, an eye placed at E receives the same impression as if the object O P, from which the rays originally proceed, existed at *o*. It is of great importance, then, to know, as in the preceding cases, the place and the magnitude of this imaginary image *o p*. As to the place, it is sufficient to remark, that if the distance of the object A O be equal to the distance of the focus of the lens, the image will be at an infinite distance from it; and this is what the present case has in common with the preceding; but the nearer the object is brought to the lens, or the less that the distance A O becomes than that of the focus of the lens, the nearer does the imaginary image approach to the lens; though, at the same time, it remains always at a greater distance from the lens than the object itself.

To elucidate this by an example, let us suppose that the focal distance of the lens is 6 inches; and for the different distances of the object, the annexed table indicates the distance of the imaginary image *o p*.

If the distance of the Object A O is	*The distance of the imaginary Image A o will be*
6	Infinity
5	30
4	12
3	6
2	3
1	1 and a fifth.

The rule for ascertaining the magnitude of this imaginary image *o p* is easy and general; you have only to draw through the middle of the lens, marked C, and through the extremity of the object P, the straight line C P *p*; and where it meets with the line *o p* drawn from *o* at right angles with the axis of the lens, you will have found the magnitude of the imaginary image *o p*: from which it is evident, that this image is always greater than the object O P itself, as many times as it is farther from the lens than the object O P. It is likewise evident that this image is not reversed, as in the preceding case, but upright as the object.

You will easily comprehend, from what I have said, the benefit that may be derived from lenses of this sort, by persons whose sight is not adapted to the view of near objects, but who can see them to more advantage at a considerable distance. They have only to look at objects through a convex lens, in order to see them as if they were very distant. The defect of sight with respect to near objects occurs usually in aged people, who consequently make use of spectacles with convex glasses, which, exposed to the sun produce the effect of a burning-glass, and this ascertains the focal distance of every glass. Some persons have occasion for spectacles of a very near focus, others of one more distant, according to the state of their sight; but it is sufficient for my present purpose to have given a general idea of the use of such spectacles.

12th January, 1762.

LETTER LXXXIII.

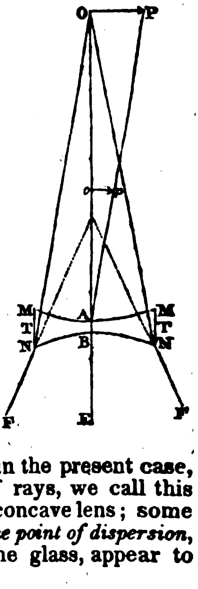
Use and Effect of a Concave Lens.

You have seen how convex glasses assist the sight of old people, by representing to them objects as at a greater distance than they really are; there are eyes, on the contrary, which, in order to distinct vision, require the objects to be represented as nearer; and concave glasses procure them this advantage; which leads me to the explanation of the

effect of concave lenses, which is directly the contrary of that of convex ones.

When the object O P, *Fig.* 157, is very distant, and its rays O M, O M, fall almost parallel on the concave lens T T; in this case, instead of becoming convergent by the refraction of the lens, they, on the contrary, become more divergent, pursuing the direction N F, N F, which, produced backward, meet at the point *o*; so that an eye placed, for example, at E, receives these refracted rays in the same manner as if they proceeded from the point *o*, though they really proceed from the point O; for this reason, I have in the figure dotted the straight lines N *o*, N *o*.

Fig. 157.



As the object is supposed to be infinitely distant, were the lens convex the point *o* would be what we call the focus; but as, in the present case, there is no real concurrence of rays, we call this point the imaginary focus of the concave lens; some authors likewise denominate it *the point of dispersion*, because the rays, refracted by the glass, appear to be dispersed from this point.

Concave lenses, then, have no real focus, like the convex, but only an imaginary focus, the distance of which from the lens A *o* is, however, denominated the focal distance of this lens, and serves, by means of a rule similar to that which is laid down for convex lenses, to determine the place of the image, when the object is not infinitely distant. Now, this image is always imaginary; whereas in the case of convex lenses, it becomes so only when the object is nearer than the distance of the focus. Without entering into the explanation of this rule, which respects calculation merely, it is sufficient to remark:—

1. When the object O P is infinitely distant, the imaginary image *o p* is represented at the focal distance of the concave lens, and this, too, on the same side with the object. Nevertheless, though this image be imaginary, the eye placed at E is quite as much affected by it as if it were real, conformably to the explanation given on the subject of convex lenses, when the object is nearer the lens than its focal distance.

2. On bringing the object O P nearer to the lens, its image *o p* will likewise approach nearer, but in such a manner that the image will always be nearer to the lens than the object is; whereas, in the case of convex lenses, the image is more distant from the lens than the object. In order to elucidate this more clearly, let us suppose the focal distance of the concave lens to be 6 inches.

If the Distance of the Object O A *is*	*The Distance of the Image* *o* A *will be*
Infinite.	6
30	5
12	4
6	3
3	2
2	1 and a half.

3. By the same rule you may always determine the magnitude of the imaginary image *o p*. You draw from the middle of the lens a straight line, to the extremity of the object P, which will pass through the extremity *p* of the image. For, since the line P A represents a ray coming from the extremity of the object, this same ray must, after the refraction, pass through the extremity of the image; but as

this ray PA passes through the middle of the lens, it undergoes no refraction; therefore it must itself pass through the extremity of the image, at the point *p*.

4. This image is not reversed, but in the same position with the object; and it may be laid down as a general rule, that whenever the image falls on the same side of the lens that the object is, it is always represented upright, whether the lens be convex or concave; but when represented on the other side of the lens, it is always reversed; and this can take place only in convex lenses.

5. It is evident therefore that the images represented by concave lenses are always smaller than the objects; the reason is obvious—the image is always nearer than the object; you have only to look at the figure to be satisfied of this truth. These are the principal properties to be remarked respecting the nature of concave lenses, and the manner in which objects are represented by them.

It is now easy to comprehend how concave glasses may be rendered essentially serviceable to persons whose sight is short. You are acquainted with some who can neither read nor write without bringing the paper almost close to their nose. In order, therefore, to their seeing distinctly, the object must be brought very near to the organ of vision: I think I have formerly remarked that such persons are denominated *myopes*. Concave lenses, then, may be made of great use to them, for they represent the most distant objects as very near; the image not being farther from such glasses than their focal distance, which, for the most part, is only a few inches.

These images, it is true, are much smaller than the objects themselves; but this by no means prevents distinctness of vision. A small object near may appear greater than a very large body at a distance. In fact, the head of a pin appears to the eye greater than a star in the heavens, though that star far exceeds the earth in magnitude.

Persons whose sight is short, or *myopes*, have occasion, then, for glasses which represent objects as nearer; such are concave lenses. And those whose sight is long, or *presbytes*, need convex glasses, which represent to them objects at a greater distance.

16th January, 1762.

LETTER LXXXIV.

Of apparent Magnitude, of the Visual Angle, and of Microscopes in general.

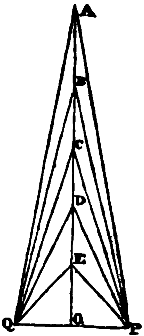
I HAVE been remarking, that *myopes* are obliged to make use of concave glasses to assist their vision of distant objects, and that *presbytes* employ convex glasses in order to a more distant vision of such as are near; each sight has a certain extent, and each requires a glass which shall represent objects perfectly. This distance in the *myopes* is very small, and in the *presbytes* very great; but there are eyes so happily conformed as to see nearer and more distant objects equally well.

Nevertheless, of whatever nature any person's sight may be, this distance is never very small: there is no *myope* capable of seeing distinctly at the distance of less than an inch; you must have observed, that when the object is brought too close to the eye, it has a very confused appearance; this depends on the structure of the organ, which is such in the human species as not to admit of their seeing objects very near. To insects, on the contrary, very distant objects are invisible, while they easily see such as are nearer. I do not believe that a fly is capable of seeing the stars, because it can see extremely well at the distance of the tenth part of an inch, a dis-

tance at which the human eye can distinguish absolutely nothing. This leads me to an explanation of the microscope, which represents to us the smallest object as if it were very great.

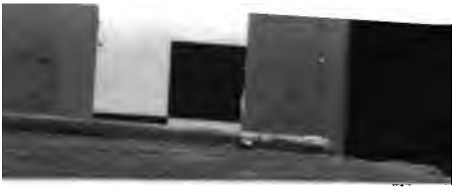
In order to convey a just idea of it, I must entreat you carefully to distinguish between the apparent and the real magnitude of every object. Real magnitude constitutes the object of geometry, and is invariable as long as the body remains in the same state. But apparent magnitude admits of infinite variety, though the body may remain always the same. The stars, accordingly, appear to us extremely small, though their real magnitude is prodigious, because we are at an immense distance from them. Were it possible to approach them, they would appear greater; from which you will conclude that the apparent magnitude depends on the angle formed in our eyes by the rays which proceed from the extremities of the object.

Fig. 158.



Let P O Q, *Fig.* 158, be the object of vision, which, if the eye were placed at A, would appear under the angle P A Q, called the visual angle, and which indicates to us the apparent magnitude of the object; it is evident, on inspecting the figure, that the farther the eye withdraws from the object, the smaller this angle becomes, and that it is possible for the greatest bodies to appear to us under a very small visual angle, provided our distance from them be very great, as is the case with the stars. But when the eye approaches nearer to the object, and looks at it from B, it will appear under the visual angle P B Q, which is evidently greater than P A Q. Let the eye advance still forward to C, and the visual angle P C Q is still greater. Further, the eye being placed at D, the visual angle will be P D Q; and on advancing forward to E, the visual angle will be P E Q, always greater and greater. The nearer, therefore, the eye approaches to the object, the more the visual angle increases, and consequently likewise the apparent magnitude. However small the object may be, it is possible, therefore, to increase its apparent magnitude at pleasure; you have only to bring it so near the eye as is necessary to form such a visual angle. A fly near enough to the eye may, of consequence, appear under an angle as great as an elephant at the distance of ten feet. In a comparison of this sort, we must take into the account the distance at which we suppose the elephant to be viewed; unless this is done, we affirm absolutely nothing; for an elephant appears great only when we are not very far from it; at the distance of a mile, it would be impossible, perhaps, to distingush an elephant from a pig; and, transported to the moon, he would become absolutely invisible; and I might affirm with truth, that a fly appeared to me greater than an elephant, if the latter was removed to a very considerable distance. Accordingly, if we would express ourselves with precision, we must not speak of the apparent magnitude of a body, without taking distance likewise into the account, as the same body may appear very great or very small according as its distance is greater or less. It is very easy, then, to see the smallest bodies under very great visual angles; they need only to be placed very close to the eye.

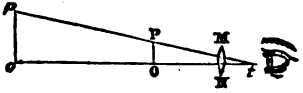
This expedient may be well enough adapted to a fly, but the human eye could see nothing at too small a distance, however short the sight may be; besides, persons of the best sight would wish to see likewise the smallest objects extremely magnified. The thing required, then, is to find the means of enabling us to view an object distinctly, notwithstanding its great proximity to the eye. Convex lenses



render us this service, by removing the image of objects which are too near.

Let a very small convex lens M N be employed, *Fig.* 159, the focal distance of which shall be half an inch; if you place before it a small object O P, at a distance somewhat less than half an inch, the lens will represent the image of it *o p*, as far off as could be wished. On placing the eye, then, behind the lens, the object will be seen as if it were at *o*, and at a sufficient distance, as if its magnitude were *o p*: as the eye is supposed very near the lens, the visual angle will be *p t o*, that is, the same as P *t* O, under which the naked eye would see the object O P in that proximity; but the vision is become distinct by means of the lens: such is the principle on which microscopes are constructed.

Fig. 159.



19th January, 1762.

LETTER LXXXV.

Estimation of the Magnitude of Objects viewed through the Microscope.

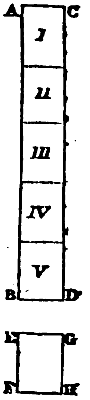
When several persons view the same object through a microscope, the foot of a fly, for example, they all agree that they see it greatly magnified, but their judgment respecting the real magnitude will vary; one will say, it appears to him as large as that of a horse; another, as that of a goat; a third, as that of a cat. No one then advances any thing positive on the subject, unless he adds at what distance he views the feet of the horse, the goat, or the cat.

They all mean, therefore, without expressing it, a certain distance, which is undoubtedly different; consequently, there is no reason to be surprised at the variety of the judgments which they pronounce, as the foot of a horse viewed at a distance, may very well appear no bigger than that of a cat viewed near to the eye. Accordingly, when the question is to be decided, How much does the microscope magnify an object? we must accustom ourselves to a more accurate mode of expression, and particularly to specify the distance, in the comparison which we mean to institute.

It is improper, therefore, to compare the appearances presented to us by the microscope with objects of another nature, which we are accustomed to view sometimes near and sometimes at a distance. The most certain method of regulating this estimation seems to be that which is actually employed by authors who treat of the microscope. They compare a small object viewed through the microscope with the appearance which it would present to the naked eye on being removed to a certain distance; and they have determined, that in order to contemplate such a small object to advantage by the naked eye, it ought to be placed at the distance of eight inches, which is the standard for good eyes, for a short-sighted person would bring it closer to the eye, and one far-sighted would remove it. But this difference does not affect the reasoning, provided the regulating distance be settled; and no reason can be assigned for fixing on any other distance than that of eight inches, the distance received by all authors who have treated of the subject. Thus, when it is said that a microscope magnifies the object a hundred times, you are to understand that, with the assistance of such a microscope, objects appear a hundred times greater than if you viewed them at the distance of eight inches; and thus you will form a just idea of the effect of a microscope.

In general, a microscope magnifies as many times as an object appears larger than if it were viewed without the aid of the glass at the distance of eight inches. You will readily admit that the effect is surprising, if an object is made to appear even a hundred times greater than it would to the naked eye at the distance of eight inches: but it has been carried much farther; and microscopes have been constructed which magnify five hundred times—a thing almost incredible. In such a case it might be with truth affirmed that the leg of a fly appears greater than that of an elephant. Nay, I have full conviction that it is possible to construct microscopes capable of magnifying one thousand, or even two thousand times, which would undoubtedly lead to the discovery of many things hitherto unknown.

Fig. 160.



But when it is affirmed that an object appears through the microscope a hundred times greater than when viewed at the distance of eight inches, it is to be understood that the object is magnified as much in length as in breadth and depth, so that each of these dimensions appears a hundred times greater. You have only, then, to conceive at the distance of eight inches another object similar to the first, but whose length is a hundred times greater, as well as its breadth and depth, and such will be the image viewed through the microscope. Now, if the length, the breadth, and depth of an object be a hundred times greater than those of another, you will easily perceive that the whole extent will be much more than a hundred times greater. In order to put this in the clearest light, let us conceive two parallelograms A B C D, and E F G H, *Fig.* 160, of the same breadth, but that the length of the first, A B, shall be five times greater than the length of the other, E F; it is evident that the area, or space contained in the first, is five times greater than that contained in the other, as in fact this last is contained five times in the first. To render, then, the parallelogram A D five times greater than the parallelogram E H, it is sufficient that its length A B be five times greater, the breadth being the same; and if, besides, the breadth were likewise five times greater, it would become five times greater still, that is, five times five times, or twenty-five times greater. Thus, of two surfaces, if the one be five times longer and five times broader than the other, it is in fact twenty-five times greater.

If we take, further, the height or depth into the account, the increase will be still greater. Conceive two apartments, the one of which is five times longer, five times broader, and five times higher than the other; its contents will be five times 25 times, that is, 125 times greater. When, therefore, it is said that a microscope magnifies 100 times, as this is to be understood, not only of length, but of breadth, and depth, or thickness, that is, of three dimensions, the whole extent of the object will be increased 100 times 100 times 100 times; now 100 times 100 make 10,000, which taken again 100 times make 1,000,000; thus, when a microscope magnifies 100 times, the whole extent of the object is represented 1,000,000 times greater. We satisfy ourselves, however, with saying that the microscope magnifies 100 times; but it is to be understood that all the three dimensions, namely, length, breadth, and depth, are represented 100 times greater. If, then, a microscope should magnify 1000 times, the whole extent of the object would become 1000 times 1000 times 1000 times greater, which makes 1,000,000,000, or a thousand millions: a most astonishing effect! This remark is necessary to the formation of a just idea of what is said respecting the power of microscopes.*

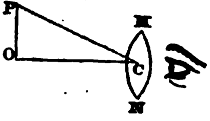
23*d January*, 1762.

LETTER LXXXVI.

Fundamental Proposition for the Construction of Simple Microscopes. Plan of some Simple Microscopes.

Having explained in what manner we are enabled to judge of the power of microscopes, it will be easy to unfold the fundamental principle for the construction of simple microscopes. And here it may be necessary to remark, that there are two kinds of microscopes; some consisting of a single lens, others of two or more, named, accordingly, simple or compound microscopes, and which require particular elucidations. I shall confine myself at present to the simple microscope, which consists of a single convex lens, the effect of which is determined by the following proposition: *A simple microscope magnifies as many times as its focal distance is nearer than eight inches.* The demonstration follows.

Fig. 161.



Let M N, *Fig.* 161, be a convex lens, whose focal distance, at which the object O P must be placed nearly, in order that the eye may see it distinctly, shall be C O; this object will be perceived under the angle O C P. But if it be viewed at the distance of eight inches, it would appear under an angle as many times smaller as the distance of eight inches surpasses the distance C O: the object will appear, therefore, as many times greater than if it were viewed at the distance of eight inches. Now, in conformity to the rule already established, a microscope magnifies as many times as it presents the object greater than if we viewed it at the distance of eight inches. Consequently, a microscope magnifies as many times as its focal distance is less than eight inches. A lens, therefore, whose focal distance is an inch will magnify precisely eight times; and a lens whose focal distance is only half an inch will magnify sixteen times. The inch is divided into twelve parts, called *lines;* half an inch, accordingly, contains six lines: hence it would be easy to determine how many times every lens, whose focal distance is given in lines, must magnify; according to the following table:—

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* As it is in reality only the surface of bodies that is presented to the eye, it may be questioned whether the magnifying power of a microscope ought to be estimated at a higher rate than that of the square; thus, if it magnify 100 times in length, the object will appear 10,000 times greater than to the naked eye.—*Am. Ed.*

Focal distance of the lens in lines.

	12,	8,	6,	4,	3,	2,	1,	½	lines,
magnifies	8,	12,	16,	24,	32,	48,	96,	192	times.

Thus a convex lens whose focal distance is one line magnifies *ninety-six times;* and if the distance be half a line, the microscope will magnify *one hundred and ninety-two*, that is, near two hundred times. Were greater effect still to be desired, lenses must be constructed of a still smaller focus.* Now, it has been already remarked, that in order to construct a lens of any certain given focus, it is only necessary to make the radius of each face equal to that focal distance, so that the lens may become equally convex on both sides. I now proceed, then, to place before you, *Fig.* 162, the form of some of these lenses or microscopes:—

No. I. The focal distance of this lens A O is one inch, or twelve lines. This microscope, therefore, magnifies eight times.

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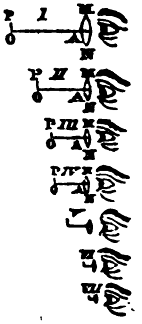
* Lenses have been ground and polished having only a focal length of one-fiftieth of an inch, consequently their magnifying power is 400 times.—*Ed.*

Fig. 162.



No. II. The focal distance of the lens M N is eight lines. This microscope magnifies twelve times.

No. III. The focal distance of the lens M N is six lines. This microscope magnifies sixteen times.

No. IV. The focal distance of this lens is four lines; and such a microscope magnifies twenty-four times.

No. V. The focal distance here is three lines. This microscope magnifies thirty-two times.

No. VI. The focal distance here is two lines. This microscope magnifies forty-eight times.

No. VII. The focal distance of this lens is only one line; and such a microscope magnifies ninety-six times.

It is possible to construct microscopes still much smaller. They are actually executed, and much more considerable effects are produced; whence it must be carefully remarked, that the distance of the object from the glass becomes smaller and smaller, as it must be nearly equal to the focal distance of the lens. I say *nearly*, as every eye brings the glass closer to it somewhat more or less, according to its formation; the short-sighted apply it closer, the far-sighted less so. You perceive, then, that the effect is greater as the microscope or lens becomes smaller, and the closer likewise the object must be applied: this is a very great inconvenience, for, on the one hand, it is troublesome to look through a glass so very small; and, on the other, because the object must be placed so near the eye. Attempts have been made to remedy this inconvenience by a proper mounting, which may facilitate the use of it; but the vision of the object is considerably disturbed as soon as the distance of it undergoes the slightest change: and as in the case of a very small lens the object must

almost touch it, whenever the surface of the object is in the least degree unequal, it is seen but confusedly. For, while the eminences are viewed at the just distance, the cavities, being too far removed, must be seen very confusedly. This renders it necessary to lay aside simple microscopes when we wish to magnify very considerably, and to have recourse to the compound microscope.

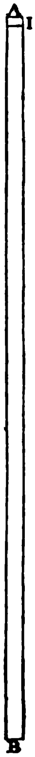
26th January, 1762.

LETTER LXXXVII.

Limits and Defects of the Simple Microscope.

You have now seen how simple microscopes may be constructed, which shall magnify as many times as may be desired; you have only to measure off a straight line of eight inches, like that which I have marked A B,* *Fig.* 163, which contains precisely eight inches of the Rhenish foot, which is the standard all over Germany. This line A B must then be subdivided into as many equal parts as correspond to the number of times you wish to magnify the object proposed, and one of these parts will give the focal distance of the lens that is requisite. Thus, if you wish to magnify a hundred times, you must take the hundredth part of the line A B; consequently, you must construct a lens whose focal distance shall be precisely equal to that part A I, which will give, at the same time, the radius of the surfaces

Fig. 163.



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* It being impossible here to insert a straight line of eight inches, one of half that length is employed, for the purpose of illustration.

of the lens represented in No. VII. of the preceding figure. Hence it is evident, that the greater the effect we mean to produce, the smaller must be the lens, as well as the focal distance at which the object O P must be placed before the lens, while the eye is applied behind it: and if the lens were to be made twice smaller than what I have now described, in order to magnify two hundred times, it would become so minute as almost to require a microscope to see the lens itself; besides, it would be necessary to approach so close as almost to touch the lens, which, as I have already observed, would be very inconvenient. The effect of the microscope, therefore, could hardly be carried beyond two hundred times; which is by no means sufficient for the investigation of many of the minuter productions of nature. The purest water contains small animalcules, which, though magnified two hundred times, still appear no bigger than fleas; and a microscope which should magnify 20,000 times would be necessary to magnify their appearance to the size of a rat; and we are far from reaching this degree, even with the assistance of the compound microscope.*

But besides the inconveniences attending the use of simple microscopes which have been already pointed out, all those who employ them with a view to very great effect complain of another considerable defect; it is this—the more that objects are magnified, the more obscure they appear; they seem as if viewed in a very faint light or by moonlight, so that you can hardly distinguish any thing clearly. You will not be surprised at this, when you recollect that the light of the full moon is more than two hundred thousand times fainter than that of the sun.

It is of much importance, therefore, to explain whence this diminution of light proceeds. We can easily comprehend, that if the rays which proceed from a very small object must represent it to us as if it were much larger, this small quantity of light would not be sufficient. But however well founded this reasoning may appear, it wants solidity, and throws only a false light on the question. For if the lens, as it proceeded in magnifying, necessarily produced a diminution of clearness, this must likewise be perceptible in the smallest effects, even supposing it were not to so high a degree; but you may magnify up to fifty times, without perceiving the least apparent diminution of light, which, however, ought to be fifty times fainter, if the reasons adduced were just. We must look elsewhere, then, for the cause of this phenomenon, and even resort to the first principles of vision.

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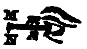
* It is not probable that water perfectly pure contains any animalculæ,—that is, water prepared by the slow and careful distillation of clear fresh rain-water, and preserved in close vessels.—*Am. Ed.*

I must entreat you, then, to recollect what I have already suggested respecting the use of the pupil, or that black aperture which we see in the eye at the middle of the iris. It is through this aperture that the rays of light are admitted into the eye; accordingly, the larger this aperture is, the more rays are admitted. We must here consider two cases in which objects are very luminous and brilliant, and in which they are illuminated by only a very faint light. In the first, the pupil contracts of itself, without any act of the will; and the Creator has bestowed on it this faculty in order to preserve the interior of the eye from the too dazzling effect of light, which would infallibly injure the nerves. Whenever, therefore, we are exposed to a very powerful light, we observe that the pupil of every eye contracts, to prevent the admission of any more rays into the eye than are necessary to paint in it an image sufficiently luminous. But the contrary takes place when we are in the dark; the pupil in that case expands, to admit the light in a greater quantity. This change is easily perceptible every time we pass from a dark to a luminous situation. With respect to the subject before us, I confine myself to this circumstance, that the more rays of light are admitted into the eye, the more luminous will be the image transmitted to the retina; and reciprocally, the smaller the quantity of rays which enter the eye, the fainter does the image become, and, consequently, the more obscure does it appear. It may happen, that though the pupil is abundantly expanded, a few rays only shall be admitted into the eye. You have only to prick a little hole in a card with a pin, and look at an object through it; and then, however strongly illuminated by the sun, the object will appear dark in proportion as the aperture is small; nay, it is possible to look at the sun itself, employing this precaution. The reason is obvious, a few rays only are admitted into the eye; however expanded the pupil may be, the pin-hole in the card determines the quantity of light which enters the eye, and not the pupil, which usually performs that function.

The same thing takes place in the microscopes which magnify very much; for when the lens is extremely small, a very few rays only are transmitted, as *m n*, *Fig.* 165, which being smaller than the aperture of the pupil, make the object appear so much more obscure; hence it is evident that this diminution of light takes place only when the lens M N, or rather its open part, is smaller than the pupil. If it were possible to produce a great magnifying effect, by means of a greater lens, this obscurity would not take place; and this is the true solution of the question. In order to remedy this inconvenience in the great effects of the microscope, care is taken to illuminate the object as strongly as possible, to give greater force to the few rays which are conveyed into the eye. To this effect objects are illuminated by the sun itself; mirrors likewise are employed, which reflect on them the light of the sun. These are

Fig. 165.



nearly all the circumstances to be considered respecting the simple microscope, and by these you will easily form a judgment of the effect of all those which you may have occasion to inspect.*

30*th January*, 1762.

LETTER LXXXVIII.

On Telescopes, and their Effect.

Before I proceed to explain the construction of compound microscopes, a digression respecting the telescope may perhaps be acceptable. These two instruments have a very intimate connexion; the one greatly assists the elucidation of the other. As microscopes serve to aid us in contemplating nearer objects, by representing them under a much greater angle than when viewed at a certain distance, say *eight* inches; so the telescope is employed to assist our observation of very distant objects, by representing them under a greater angle than that which they present to the naked eye. Instruments of this sort are known by several names, according to their size and use; but they must be carefully distinguished from the glasses used by aged persons to relieve the decay of sight.

A telescope magnifies as many times as it represents objects under an angle greater than is presented to the naked eye. The moon, for example, appears to the naked eye under an angle of half a degree; consequently, a telescope magnifies 100 times when it represents the moon under an angle of fifty degrees, which is 100 times greater than half a degree. If it magnified 200 times, it would represent the moon under an angle of one hundred degrees; and the moon would in that case appear to fill more than half of the visible heavens, whose whole extent is only 180 degrees.*

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* For an account of various improvements on the single microscope, the reader is referred to the article *Optics*, in the *Edinburgh Encyclopædia*, vol. xv. p 631, and *Ferguson's Lectures*, vol. ii. p. 294.—*Ed.*

For still later improvements, see a paper by *Dr. Roget*, in *Phil Transactions*, for May, 1830.—*Am. Ed.*

In common language, we say that the telescope brings the object nearer to us. This is a very equivocal mode of expression, and admits of two different significations. The one, that on looking through a telescope, we consider the object as many times nearer as it is magnified. But I have already remarked, that it is impossible to know the distance of objects but by actual measurement, and that such measurement can be applied only to objects not greatly remote; when, therefore, they are so remote as is here supposed, the estimation of distance might greatly mislead us. The other signification, which conveys the idea that telescopes represent objects as great as they would appear if we approached nearer to them, is more conformable to truth. You know that the nearer we come to any object, the greater becomes the angle under which it appears; this explanation, accordingly, reverts to that with which I set out. When, however, we look at well-known objects, say men, at a great distance, and view them through a telescope under a much greater angle, we are led to imagine such men to be a great deal nearer, as in that case we would, in effect, see them under an angle so much greater. But in examining objects less approachable, such as the sun and moon, no measurement of distance can take place. This case is entirely different from that which I have formerly submitted to you, that of a concave lens, employed by near-sighted persons, which represents the images of objects at a very small distance. The concave lens which I use, for example, represents to me the images of all remote objects at the distance of four inches; it is impossible for me, however, to imagine that the sun, moon, and stars are so near: accordingly, we do not conclude that objects are where their images are found represented by glasses; we believe this as little as we do the existence of objects in our eyes, though their images are painted there. You will please to recollect, that the estimation of the real distance and real magnitude of objects depends on particular circumstances.

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* The magnifying power is ascertained by measuring the aperture of the object-glass, and that of the little image of it which is formed at the end of the eye-piece; the proportion between these will give the ratio of the magnifying power.

When single lenses are used, the power of a glass is readily discovered by dividing the focal length of the object-glass by that of the eye-glass.—*Am. Ed.*

The principal purpose of telescopes, then, is to increase, or multiply, the angle under which objects appear to the naked eye; and the principal division of telescopes is estimated by the effect which they procure. Accordingly, we say such a telescope magnifies five, another ten, another twenty, another thirty times, and so on. And here I remark, that pocket-glasses rarely magnify beyond *ten* times; but the usual telescopes employed for examining very distant terrestrial objects magnify from *twenty* to *thirty* times, and their length amounts to *six* feet or more. A similar effect, though very considerable with regard to terrestrial objects, is a mere nothing with respect to the heavenly bodies, which require an effect inconceivably greater. We have, accordingly, astronomical telescopes which magnify from 50 to 200 times; and it would be difficult to go further, as, according to the usual mode of constructing them, the greater the effect is the longer they become. A telescope that shall magnify 100 times must be at least 30 feet long: and one of 100 feet in length could scarcely magnify 200 times. You must be sensible, therefore, that the difficulty of pointing and managing such an unwieldy machine, must oppose insurmountable obstacles to pushing the experiment further. The famous Hevelius, the

astronomer at Dantzic, employed telescopes 200 feet long; but such instruments must undoubtedly have been very defective, as the same things are now discovered by instruments much shorter.

This is a brief general description of telescopes, and of the different kinds of them, which it is of importance carefully to remark, before we enter into a detail of their construction, and of the manner in which two or more lenses are united, in order to produce all the different effects.

2d February, 1762.

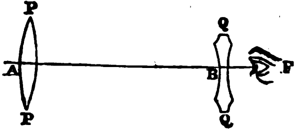
LETTER LXXXIX.

Of Pocket-glasses.

We have no certain information respecting the person to whom we are indebted for the discovery of the telescope: whether he were a Dutch artist, or an Italian of the name of Porta.[*] Whoever he was, it is almost one hundred and fifty years since small pocket-glasses were first constructed, composed of two lenses, of which the one was convex, and the other concave. To pure chance, perhaps, a discovery of so much utility is to be ascribed. It was possible, without design, to place two lenses nearer to or farther from each other, till the object appeared distinctly.

The convex lens P A P, *Fig.* 166 is directed towards

Fig. 166.



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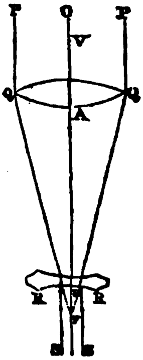
[*] If the telescope was not actually invented by *Roger Bacon*, or *Leonard Digges*, they at least constructed combinations of lenses and mirrors which produced the same effect.—*Ed.*

the object, and the eye is applied to the concave lens Q B Q; for which reason, the lens P A P is named the *object-glass*, and Q B Q the *eye-glass*. These two lenses are disposed on the same axis A B, perpendicular to both, and passing through their centres. The focal distance of the convex lens P A P must be greater than that of the concave; and the lenses must be disposed in such a manner, that if A F be the focal distance of the objective P A P, the focus of the eye-glass Q Q B must fall at the same point F; accordingly, the interval between the lenses A and B is the difference between the focal distances of the two lenses, A F being the focal distance of the object-glass, and B F that of the eye-glass. When the lenses are arranged, a person with good eyes will clearly see distant objects, which will appear as many times greater as the line A F is greater than B F. Thus, supposing the focal distance of the object-glass to be six inches, and that of the eye-glass one inch, the object will be magnified six times, or will appear under an angle six times greater than when viewed with the naked eye; and, in this case, the interval between the lenses A, B will be five inches, which is, at the same time, the length of the instrument. There is no need to inform you that these two lenses are cased in a tube of the same length, though not thus represented in the figure.

Having shown in what manner the two lenses are to be joined together, in order to produce a good instrument, two things must be explained to you: the one, How these lenses come to represent objects distinctly; and the other, Why they appear magnified as many times as the line A F exceeds the line B F. With respect to the first, it must be remarked, that a good eye sees objects best, when they are so distant that the rays which fall on the eye may be considered as parallel to each other.

Let us consider, then, a point V, *Fig.* 167, in the object towards which the instrument is directed, and on the supposition of its being very distant, the rays

Fig. 167.

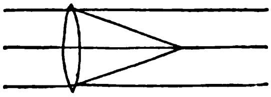


which fall on the object-glass P Q, O A, P Q, will be almost parallel to each other; accordingly, the object-glass, Q A Q, being a convex lens, will collect them in its focus F, so that these rays, being convergent, will not suit a good eye. But the concave lens at B, having the power of rendering the rays more divergent, or of diminishing their convergency, will refract the rays Q R, Q R, so that they shall become parallel to each other; that is, instead of meeting in the point F, they will assume the direction R S, R S, parallel to the axis B F. Thus a good eye, according to which the construction of these is always regulated, on receiving these parallel rays R S, B F, R S, will see the object distinctly. The rays R S, R S become exactly parallel to each other, because the concave lens has its focus, or rather its point of dispersion, at F.

You have only to recollect, that when parallel rays fall on a concave lens, they become divergent by refraction, so that being produced backward, they meet in the focus. This being laid down, we have only to reverse the case, and to consider the rays S R, S R, as falling on the concave lens: in this case it is certain they would assume the directions R Q, R Q, which produced backwards would meet in the point F, which is the common focus of the convex and concave lenses. Now it is a general law, that in whatever manner rays are refracted in their passage from one place to another, they must always undergo the same refractions in returning from the last to the first. If, therefore, the refracted rays R Q, R Q correspond to the incident rays S R, S R; then, reciprocally, the rays Q R, Q R, being the incident ones, the refracted rays will be R S and R S.

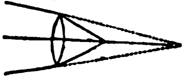
The matter will perhaps appear in a clearer light still, when I say that concave lenses have the power of rendering parallel those rays which, without the refraction, would proceed to their focus. You will please carefully to attend to the following laws of refraction, which apply to both convex and concave lenses.

Fig. 168.



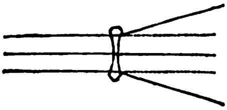
1. By a convex lens, *Fig*. 168, parallel rays are rendered convergent.

Fig. 169.



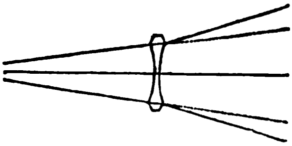
Convergent rays become still more so, *Fig*. 169, and divergent less divergent.

Fig. 170.



2. By a concave lens parallel rays are rendered divergent. *Fig*. 170.

Fig. 171.



Divergent rays become still more divergent, *Fig*. 171, and convergent rays less convergent.

All this is founded on the nature of refraction and the figure of the lenses, the discussion of which would require a very long detail; but the two rules which I have now laid down contain all that is essential. It is abundantly evident, then, that when the convex and the concave lenses are so combined that they acquire a common focus at F, they will distinctly represent distant objects, because the parallelism of the rays is restored by the concave lens after the convex lens had rendered them convergent. In other words, the rays of very distant objects, being nearly parallel to each other, become convergent by a convex lens; and afterward, the concave lens destroys this convergency, and again renders the rays parallel to each other.

6th February, 1762.

LETTER XC.

On the magnifying Power of Pocket-glasses.

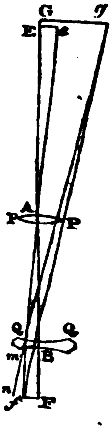
The principal article respecting telescopical instruments remains still to be explained, namely, their effect in magnifying objects. I hope to place this in so clear a light as to remove every difficulty in which the subject may be involved; and for this purpose I shall comprise what I have to say in the following propositions.

1. Let E *e*, *Fig.* 172, be the object, situated on the axis of the instrument, which passes perpendicularly through both lenses in their centres. This object E *e* must be considered as at an infinite distance.

2. If, then, the eye, placed at A, looks at this object, it will appear under the angle E A *e*, called its visual angle. It will, accordingly, be necessary to prove, that on looking at the same object through the glass it will appear under a greater angle, and exactly as many times greater as the focal distance of the object-glass P A P exceeds that of the eye-glass Q B Q.

3. As the effect of all lenses consists in representing the objects in another place, and with a certain magnitude, we have only to examine the images which shall be successively represented by the

Fig. 172.



two lenses, the last of which is the immediate object of the sight of the person who looks through the instrument.

4. Now, the object E *e* being infinitely distant from the convex lens P A P, its image will be represented behind the lens at F *f*, so that A F shall be equal to the focal distance of the lens; and the magnitude of this image F *f* is determined by the straight line *f* A *e*, drawn from the extremity of the object *e*, through the centre of the lens A, by which we see that this image is inverted, and as many times smaller than the object as the distance A F is smaller than the distance A E.

5. Again, this image F *f* holds the place of the object relatively to the eye-glass Q B Q, as the rays which fall on this lens are precisely those which would almost form the image F *f*, but are intercepted in their progress by the concave lens Q B Q; so that this image is only imaginary: the effect, however, is the same as if it were real.

6. This image F *f*, which we are now considering as an object being at the focal distance of the lens Q B Q, will be transported almost to infinity by the refraction of this lens. The preceding figure marks this new image at G *g*, whose distance A G must be conceived as infinite, and the rays, refracted a second time by the lens Q B Q, will pursue the same direction as if they actually proceeded from the image G *g*.

7. This second image G *g* being, then, the object of the person who looks through the instrument, its magnitude falls to be considered. To this effect, as it is produced by the first image F *f* from the refraction of the lens Q B Q, following the general rule, we have only to draw through the centre of the lens B a straight line, which shall pass through the point *f* of the first image, and that line will mark at *g* the extremity of the second image.

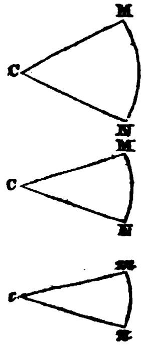
8. Let the spectator now apply his eye to B; and as the rays which it receives pursue the same

direction as if they actually proceeded from the image G *g*, it will appear to him under the angle G B *g*, which is greater than the angle E A *e*, under which the object E *e* appears to the naked eye.

9. In order the better to compare these two angles, it is evident, first, that the angle E A *e* is equal to the angle F A *f*, being vertical angles; for the same reason, the angle G B *g* is equal to the angle F B *f*, being vertical and opposite at the point B. It remains to be proved, therefore, that the angle F B *f* exceeds the angle F A *f* as many times as the line A F exceeds the line B *f*; the former of which, A F, is the focal distance of the object-glass, and the other, B F, the focal distance of the eye-glass.

10. In order to demonstrate this, we must have recourse to certain geometrical propositions respecting the nature of sectors. You will recollect that the sector is part of a circle contained between two radii C M and C N, *Fig.* 173, and an arch or portion of the circumference M N. In a sector, then, there are three things to be considered: 1. The radius of the circle, C M or C N; 2. The quantity of the arch M N; 3. The angle M C N.

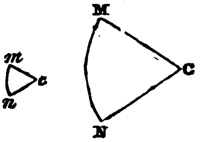
Fig. 173.



11. Let us now consider two sectors, M C N and *m c n*, whose radii C M and *c m* are equal to each other; now it is demonstrated in the elements of geometry, that the angles C and *c* have the same proportion to each other that the arches M N and *m n* have: in other words, the angle C is as many times greater than the angle *c*, as the arch M N is greater than the arch *m n*; but, instead of this awkward mode of expression, we say that the angles C and *c* are proportional to the arches M N and *m n*, the radii being equal

12. Let us likewise consider two sectors, M C N and *m c n*, *Fig.* 174, whose angles C and *c* are equal to each other, but the radii unequal: and it is demonstrated in geometry, that the arch M N is as many times greater than the arch *m n*, as the radius C M is greater than the radius *c m*; or, in geometrical language, the arches are in proportion to the radii, the angles being equal. The reason is obvious, for every arch contains as many degrees as its angle; and the degrees of a great circle exceed those of a small one as many times as the greater radius exceeds the smaller.

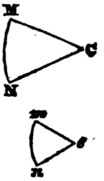
Fig. 174.



13. Finally, let us consider likewise the case when, as in the two sectors M C N and *m c n*, *Fig.* 175, the arches M N and *m n* are equal; but the radii C M and *c m* unequal.

In this case, the angle C, which corresponds to the greater radius C M, is the smaller, and the angle *c*, which corresponds to the smaller radius *c m*, is the greater; and this in the same proportion as the radii. That is, the angle *c* is as many times greater than the angle C as the radius C M is greater than the radius *c m*; or, to speak geometrically, the angles are reciprocally proportional to the radii, the arches being equal.

Fig. 175.



14. This last proposition carries me forward to my conclusion, after I have subjoined this remark, that when the angles are very small, as in the case of pocket-glasses, there is no sensible difference in the chords of the arches M N and *m n*, that is, of the straight lines M N and *m n*.

15. Having made this remark, we return to *Fig.* 172 (p. 318). The triangles F A *f* and F B *f* may be

considered as sectors, in which the arch F f is the same in both. Consequently, the angle F B f exceeds the angle F A f as often as the distance A F exceeds the distance B F. That is, the object E e will appear through the instrument under an angle as many times greater as the focal distance of the object-glass A F exceeds the focal distance of the eye-glass B F, which was the thing to be demonstrated.

9th February, 1762.

LETTER XCI.

Defects of Pocket-glasses. Of the apparent Field.

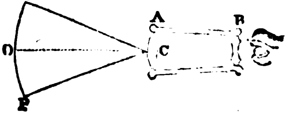
You must be sensible that no great advantage is to be expected from such small instruments; and it has already been remarked that they do not magnify objects above ten times. Were the effect to be carried further, not only would the length become too great to admit of their being carried about in the pocket, but they would become subject to other and more essential defects. This has induced artists entirely to lay aside glasses of this sort, when superior effect is required.

The principal of these defects is the smallness of the apparent field; and this leads me to explain an important article relative to telescopes of every description. When a telescope is directed towards the heavens, or to very distant objects on the earth, the space discovered appears in the figure of a circle, and we see those objects only which are included in that space; so that if you wished to examine other objects, the position of the instrument must be altered. This circular space, presented to the eye of the spectator, is denominated the *apparent field*, or, in one word, *the field* of the instrument; and it is abundantly obvious, that it must be a great advantage to have a very large field, and that, on the contrary, a small field is a very great inconvenience in instruments of this sort. Let us suppose two telescopes directed towards the moon, by the one of which we can discover only the half of that luminary, whereas by the other we see her whole body, together with the neighbouring stars; the field of this last is therefore much greater than that of the other. That which presents the greater field relieves us, not only from the trouble of frequently changing the position, but procures another very great advantage; that of enabling us to compare, by viewing them at the same time, several parts of the object one with another.

It is therefore one of the greatest perfections of a telescope to present a very ample field; and it is accordingly a matter of much importance to measure the field of every instrument. In this view, we are regulated by the heavens, and we determine the circular space seen through a telescope, by measuring its diameter in degrees and minutes. Thus, the apparent diameter of the full moon being about half a degree, if a telescope takes in the moon only, we say that the diameter of its field is half a degree; and if you could see at once only the half of the moon, the diameter of the field would be the quarter of a degree.

The measurement of angles, then, furnishes the means of measuring the apparent field; besides, the thing is sufficiently clear of itself. Supposing we could see through the instrument A B, *Fig.* 176, only the space P O P, and the objects which it contains; this space being a circle, its diameter will be the line P O P, whose middle point O is in the axis of the instrument.

Fig. 176.



Drawing, therefore, from the extremities P P the straight lines P C, P C, the angle P C P will express the diameter of the apparent field; and the half of this angle, O C P, is denominated the semi-diameter of the apparent field of such an instrument. You will perfectly comprehend the meaning, then, when it is said that the diameter of the apparent field of such an instrument is one degree, that of another two degrees, and so on; as also when it is marked by minutes, as 30 minutes, which make half a degree, or 15 minutes, which make the fourth part of a degree.

But in order to form a right judgment of the value of a telescope, with respect to the apparent field, we must likewise attend to the magnifying power of the instrument. It may be remarked in general, that the more a telescope magnifies, the smaller, of necessity, must be the apparent field; these are the bounds which nature herself has prescribed. Let us suppose an instrument which should magnify 100 times; it is evident that the diameter of the field could not possibly be so much as two degrees; for, as this space would appear 100 times greater, it would resemble a space of two hundred degrees; greater, of consequence, than the whole visible heavens, which, from the one extremity to the other, contain only 180 degrees, and of which we can see but the half at most at once,—that is, a circular space of 90 degrees in diameter. From this you see, that a telescope which magnifies 100 times could not contain a field of so much as one degree; for this degree multiplied 100 times would give more than 90 degrees; and that, accordingly, a telescope which magnified 100 times would be excellent, if the diameter of its field were somewhat less than one degree; and the very nature of the instrument admits not of a greater effect.

But another telescope which should magnify only 10 times would be extremely defective, if it discovered a field of only one degree in diameter; as this field magnified 10 times would give a space of no more than 10 degrees in the heavens, which would be a small matter, by setting too narrow bounds to our view. We should have good reason, then, to reject such an instrument altogether. Thus it would be very easy, with respect to the apparent field, to form a judgment of the excellence or defectiveness of instruments of this sort, when the effect is taken into consideration. For when it magnifies only 10 times, it may fairly be conjectured that it discovers a field of 9 degrees; as 9 degrees taken 10 times give 90 degrees, a space which our sight is capable of embracing: and if the diameter of its field were only 5 degrees or less, this would be an instrument very defective indeed. Now, I shall be able to demonstrate, that if a telescope were to be constructed such as I have been describing, which should magnify more than 10 times, it would be liable to this defect: the apparent field multiplied by the magnifying power would be very considerably under 90 degrees, and would not even show the half. But when a small effect is aimed at, this defect is not so sensible; for if such an instrument magnifies only 5 times, the diameter of its field is about 4 degrees, which magnified 5 times contains a space of 20 degrees, with which we have reason to be satisfied: but if we wished to magnify 25 times, the diameter of the field would be only half a degree, which taken 25 times would give little more than 12 degrees, which is too little. When, therefore, we would magnify very much, a different arrangement of lenses must be employed, which I shall afterward explain.

13th February, 1762.

LETTER XCII.

Determination of the apparent Field for Pocket-Glasses.

To ascertain the apparent field being of very great importance in the construction of telescopes, I proceed to the application of it to the small glasses which I have been describing.

The lens P A P, *Fig.* 172 (p. 318), is the object-glass, Q B Q the eye-glass, and the straight line E F the axis of the instrument, in which is seen, at a very great distance, through the instrument, the object E *e*, under the angle E A *e*, which represents the semi-diameter of the apparent field, for it extends as far on the other side downwards. The point E, then, is the centre of the space seen through the instrument, the radius of which, E A, as it passes perpendicularly through both lenses, undergoes no refraction; and in order that this ray may have admission into the eye, the eye must be fixed somewhere on the axis of the instrument B F, behind the eye-glass, so that the centre of the pupil shall be in the line B F; and this is a general rule for every species of telescope. Let us now consider the visible extremity of the object *e*, whose rays exactly fill the whole opening of the object-glass P A P; but it will be sufficient to attend only to the ray E A, which passes through the centre of the object-glass A, as the others surround, and little more than strengthen this ray: so that if it is admitted into the eye, the others, or at least a considerable part of them, find admission likewise; and if this ray is not admitted into the eye, though perhaps some of the others may enter, they are too feeble to excite an impression sufficiently powerful.

Hence this may be laid down as a rule, that the extremity *e* of the object is seen only so far as the ray *e* A, after having passed through the two lenses, is admitted into the eye.

We must therefore carefully examine the direction of this ray *e* A. Now, as it passes through the centre of the object-glass A, it undergoes no refraction; conformably to the rule laid down from the beginning, that rays passing through the centre of any lens whatever are not diverted from their direction, that is, undergo no refraction. This ray, *e* A, therefore, after having passed through the object-glass, would continue in the same direction, to meet the other rays issuing from the same point *e*, to the point *f* of the image represented by the object-glass at F *f*, the point *f* being the image of the point *e* of the object; but the ray meeting at *m*, the concave lens, but not in its centre, will be diverted from that direction; and instead of terminating in *f*, will assume the direction *m n*, more divergent from B F, it being the natural effect of concave lenses to render rays always more divergent. In order to ascertain this new direction *m n*, you will please to recollect that the object-glass represents the object E *e* in an inverted position at F *f*, so that A F is equal to the focal distance of this lens, which transports the object E *e* to F *f*. Then this image F *f* occupies the place of the object with respect to the eye-glass Q B Q, which, in its turn, transports that image to G *g*, whose distance B G must be as great as that of the object itself: and for this effect, it is necessary to place the eye-glass in such a manner that the interval B F shall be equal to its focal distance.

As to the magnitude of these images, the first F *f* is determined by the straight line *e* A *f*, drawn from *e* through the centre A of the first lens; and the other G *g* by the straight line *f* B *g*, drawn from the point *f* through the centre B of the second lens.

This being laid down, the ray A *m* directed towards the point *f* is refracted, and proceeds in the direction *m n*; and this line *m n*, being produced backwards, will pass through the point *g*, for *m n* has the same effect in the eye as if it actually proceeded from the point *g*. Now, as this line *m n* retires farther and farther from the axis B F, where the centre of the pupil is, it cannot enter into the eye, unless the opening of the pupil extends so far; and if the opening of the pupil were reduced to nothing, the ray *m n* would be excluded from the eye, and the point *e* of the object could not be visible, nor even any other point of the object out of the axis A F. There would, therefore, be no apparent field, and nothing would be seen through such an instrument except the single point E of the object, which is in its axis. It is evident, then, that a telescope of this sort discovers no field but as far as the pupil expands; so that in proportion as the expansion of the pupil is greater or less, so likewise the apparent field is great or small. In this case the point *e* will therefore be still visible to the eye if the small interval B *m* does not exceed half the diameter of the eye, that the ray *m n* may find admission into it; but in this case, likewise, the eye must be brought as close as possible to the eye-glass: for as the ray *m n* removes from the axis F B, it would escape the pupil at a greater distance.

Now it is easy to determine the apparent field which such an instrument would discover on the eye-glass: you have only to take the interval B *m* equal to the semi-diameter of the pupil, and to draw through that point *m*, and the centre of the object-glass A, the straight line *m* A *e*; then this line will mark on the object the extremity *e*, which will be still visible through the instrument, and the angle E A *e* will give the semi-diameter of the apparent field. Hence you will easily judge, that whenever the distance of the lenses A B exceeds some inches, the angle B A *m* must become extremely small, as the line or the distance B *m* is but about the twentieth part of an inch. Now if it were intended to magnify very much, the distance of the lenses must become considerable, and the consequence would be that the apparent field must become extremely small. The structure of the human eye, then, sets bounds to telescopes of this description, and obliges us to have recourse to others of a different construction whenever we want to produce very considerable effects.

16th February, 1762.

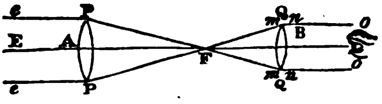
LETTER XCIII.

Astronomical Telescopes, and their magnifying Power.

I PROCEED to the second species of telescopes, called astronomical, and remark, that they consist of only two lenses, like those of the first species; with this difference, that in the construction of astronomical telescopes, instead of a concave eye-glass, we employ a convex one.

The object-glass P A P, *Fig.* 177, is, as in the other

Fig. 177.



species, convex, whose focus being at F, we place, on the same axis a smaller convex lens Q Q, in such a manner that its focus shall likewise fall on the same point F. Then placing the eye at O, so that the distance B O shall be nearly equal to the focal

distance of the eye-glass Q Q, you will see objects distinctly, and magnified as many times as the focal distance of the object-glass A F shall exceed that of the eye-glass B F: but it is to be remarked that every object will appear in an inverted position; so that if the instrument were to be pointed towards a house, the roof would appear undermost, and the ground-floor uppermost. As this circumstance would be awkward in viewing terrestrial objects, which we never see in an inverted situation, the use of this species of telescopes is confined to the heavenly bodies, it being a matter of indifference in what direction they appear; it is sufficient to the astronomer to know that what he sees uppermost is really undermost, and reciprocally. Nothing, however, forbids the application of such telescopes to terrestrial objects; the eye soon becomes accustomed to the inverted position, provided the object is seen distinctly, and very much magnified.

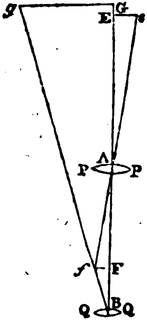
Having given this description, three things fall to be demonstrated: first, that by this arrangement of the lenses objects must appear distinctly; secondly, that they must appear magnified as many times as the focal distance of the object-glass exceeds that of the eye-glass, and in an inverted position; and thirdly, that the eye must not be applied close to the eye-glass, as in the first species, but must be removed to nearly the focal distance of the ocular.

1. As to the first, it is demonstrated in the same manner as in the preceding case: the rays *e* P, *e* P, which are parallel before they enter into the object-glass, meet by refraction in the focus of this lens at F; the eye-glass must, of course, restore the parallelism of these rays, and distinct vision requires that the rays proceeding from every point should be nearly parallel to each other when they enter the eye. Now, the eye-glass, having its focus at F, is placed in such a manner as to render the rays F M, F M, by the refraction, parallel, and consequently

the eye will receive the rays N *o*, N *o*, parallel to each other.

2. With respect to the second article, let us consider the object at E *e*, *Fig.* 178, but so that the distance E A shall be almost infinite. The image of this object, represented by the object-glass, will therefore be F *f*, situated at the focal distance of that lens A F, and determined by the straight line *e* A *f*, drawn through the centre of the lens. This image F *f*, which is inverted, occupies the place of the object with respect to the eye-glass, and being in its focus, the second image will be again removed to an infinite distance by the refraction of this lens, and will fall, for example, at G *g*, the distance A G being considered as infinite, like that of A E. Now, in order to determine the magnitude of this image, you have only to draw through the centre B of the lens, and the extremity *f* of the first image, the straight line B *f g*. Now this second image G *g* being the immediate object of vision to the person who looks through the telescope, it is evident at once that this representation is inverted, and, as it is infinitely distant, will appear under an angle G B *g*. But the object itself E *e* will appear to the naked eye under the angle E A *e*: now you are sensible, without being reminded, that it is indifferent to take the points A and B, in order to have the visual angles E A *e* and G B *g*, on account of the infinite distance of the object. You now see here, as in the preceding case, that the triangles F A *f* and F B *f* may be considered as circular sectors, the line F *f* measuring the arch of both; and the angles themselves being so very small, no sensible mistake can be committed in taking the chord for the arch. As,

Fig. 178.



then, the radii of these two sectors are the lines A E and B F, the arches being equal to each other, it follows, as was formerly demonstrated, that the angles F A *f* (or, which is the same thing, E A *e*) and F B *f* (or, which is the same thing, G B *g*) have the same proportion to each other that the radii B F and A F have. Therefore, the angle G B *g*, under which the object is seen through the telescope, as many times exceeds the angle E A *e*, under which the object is seen by the naked eye, as the line A F exceeds the line B F; which was the second point to be demonstrated. I am under the necessity of deferring the demonstration of my third proposition till next post.

20th February, 1762.

LETTER XCIV.

Of the apparent Field, and the Place of the Eye.

In fulfilling my engagement respecting the third particular proposed, namely, to determine the place of the eye behind the telescope, I remark that this subject is most intimately connected with the apparent field, and that it is precisely the field which obliges us to keep the eye fixed at the proper distance; for if it were to be brought closer, or removed farther off, we should no longer discover so large a field.

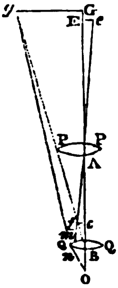
The extent of the field being an article of such importance, indeed so essential, in all telescopes, it must be of equal importance to determine exactly the place of the eye from which the largest field is discoverable. If the eye were to be applied close to the eye-glass, we should have nearly the same field as we have with the pocket-glass, which becomes insufferably small whenever the magnifying power is considerable. It is therefore a vast advantage to

astronomical telescopes, that by withdrawing the eye from the eye-glass the apparent field increases to a certain extent; and it is precisely this which renders such telescopes susceptible of prodigious magnifying powers, whereas those of the first species are in this respect extremely limited. You know that with the astronomical telescope, the magnifying power has been carried beyond two hundred times, which gives them an inconceivable superiority over those of the first species, which can scarcely magnify ten times; and the trifling inconvenience of the inverted position is infinitely overbalanced by an advantage so very great.

I will endeavour to put this important article in the clearest light possible.

1. The object E *e*, *Fig.* 179, being infinitely distant, let *e* be its extremity, still visible through the telescope, whose lenses are P A P and Q B Q, fitted on the common axis E A B O; it falls to be attentively considered what direction will be pursued by the single ray which passes from the extremity *e* of the object, through the centre A of the object-glass. You will recollect that the other rays, which fall from the point *e* on the object-glass, only accompany and strengthen the ray in question *e* A, which is the principal with respect to vision.

Fig. 179



2. Now this ray *e* A, passing through the centre of the lens P P, will undergo no refraction, but will pursue its direction in the straight line A *f m*, and passing through the extremity of the image F *f*, will fall on the eye-glass at the point *m*; and here it is to be observed, that if the size of the eye-glass had not extended so far as the point *m*, this ray would never have reached the eye, and the point *e* would have been invisible. That is to say, it would

be necessary to take the extremity *e* nearer to the axis, in order that the ray A *f m* may meet the eye-glass.

3. Now this ray A *m* will be refracted by the eye-glass in a way which it is very easy to discover. We have only to consider the second image G *g*; though infinitely distant, it is sufficient to know that the straight line B *f* produced will pass through the extremity *g* of the second image G *g*, which is the immediate object of vision. Having remarked this, the refracted ray must assume the direction *n* O, and this produced passes through *g*.

4. As, therefore, the two lines O *n* and B *f* meet at an infinite distance at *g*, they may be considered as parallel to each other; and hence we acquire an easier method to determine the position of the refracted ray *n* O: you have only to draw it parallel to the line B *f*.

5. Hence it is clearly evident that the ray *n* O will somewhere meet the axis of the telescope at O, and as usually, when the magnifying power is great, the point F is much nearer to the lens Q Q than to the lens P P, the distance B *m* will be somewhat greater than the image F *f*; and as the line *n* O is parallel to *f* B, the line B O will be nearly equal to B F, that is, to the focal distance of the eye-glass.

6. If, then, the eye is placed at O, it will receive, not only the rays which proceed from the middle of the object E, but those likewise which proceed from the extremity *e*, and consequently those also which proceed from every point of the object; the eye would even receive at once the rays B O and *n* O, even supposing the pupil infinitely contracted. In this case, therefore, the apparent field does not depend on the largeness of the aperture of the pupil, provided the eye be placed at O; but the moment it recedes from this point, it must lose considerably in the apparent field.

7. If the point *m* were not in the extremity of the

eye-glass, it would transmit rays still more remote from the axis, and the telescope would, of course, discover a larger field. In order, then, to determine the real apparent field which the telescope is capable of discovering, let there be drawn, from the centre A of the object-glass, to the extremity *m* of the eye-glass, the straight line A *m*, which, produced to the object, will mark at *e* the visible extremity; and consequently the angle E A *e*, or, which is the same thing, the angle B A *m*, will give the semi-diameter of the apparent field, which is consequently greater in proportion as the extent of the eye-glass is greater.

8. As, then, in the first species of telescopes, the apparent field depended entirely on the aperture of the pupil, and as in this case it depends entirely on the aperture of the eye-glass, there is an essential difference between these two species of instruments, greatly in favour of the latter. The figure which I have employed in demonstrating this last article respecting the place of the eye and the apparent field, may greatly assist us in the elucidation of the preceding articles.

If you will be so good as to reflect, that the object-glass transports the object E *e* to F *f*, and that the eye-glass transports it from F *f* to G *g*, this image G *g*, being very distant from the immediate object of vision, ought to be seen distinctly, as a good eye requires a great distance in order to see thus. This was the first article.

As to the second, it is evident at first sight, that as instead of the real image E *e*, we see through the telescope the image G *g*, it must be inverted. Finally, this image is seen by the eye placed at O under the angle G O *g*, or B O *n*, whereas the object itself E *e* appears to the naked eye under the angle E A *e*: the telescope, therefore, magnifies as many times as the angle B O *n* is greater than the angle E A *e*. Now, as the line *n* O is parallel to B *f*, the angle B O *n* is equal to the angle F B *f*, and the angle E A *e* is equal to its opposite and vertical angle F A *f*; hence the magnifying power must be estimated from the proportion between the angles F B *f* and F A *f*; accordingly, as the angle F B *f* contains the angle F A *f* as often as the line A F, that is, the focal distance of the object-glass, contains the line B F, that is, the focal distance of the eye-glass, the magnifying power will be therefore expressed by the proportion of these two distances. This is proof sufficient that the elements of geometry may be successfully employed in researches of quite a different nature—a reflection not unpleasing to the mathematician.

23*d February*, 1762.

LETTER XCV.

Determination of the magnifying Power of Astronomical Telescopes, and the Construction of a Telescope which shall magnify Objects a given Number of Times.

You now have it clearly ascertained, not only how many times a proposed instrument will magnify, but what is the mode of constructing a telescope which shall magnify as many times as may be wished. In the first case, you have only to measure the focal distance of both lenses, the object-glass as well as the eye-glass, in order to discover how much the one exceeds the other. This is performed by division, and the quotient indicates the magnifying power.

Having, then, a telescope, the focal distance of whose object-glass is two feet, and that of the eye-glass one inch, it is only necessary to inquire how often one inch is contained in two feet. Every one knows that a foot contains twelve inches; two feet accordingly contain twenty-four inches, which are to be divided by one. But whatever number we divide by one the quotient is always equal to the dividend; if, then, it is asked, how often one inch is contained in twenty-four inches, the answer, without hesitation, is, twenty-four times; consequently, such a telescope magnifies twenty-four times, that is, represents distant objects in the same manner as if they were twenty-four times greater than they really are; in other words, you would see them through such a telescope under an angle *twenty-four* times greater than by the naked eye.

Let us suppose another astronomical telescope, the focal distance of whose object-glass is thirty-two feet, and that of the eye-glass three inches. You see at once that these two lenses must be placed at the distance of thirty-two feet and three inches from each other; for, in all astronomical telescopes, the distance of the lenses must be equal to the sum of the two focal distances, as has been already demonstrated.

To find, then, how many times a telescope of the above description magnifies, we must divide thirty-two feet by three inches; and, in order to this, reduce these thirty-two feet into inches, by multiplying them by twelve:

$$\begin{array}{r} 32 \\ \underline{12} \\ 3)384 \\ \overline{128} \end{array}$$

this produces 384 inches; and these again divided by three, the focal distance, in inches, of the eye-glass, gives a quotient of 128, which indicates that the proposed telescope magnifies 128 times, which must be allowed to be very considerable.

Reciprocally, therefore, in order to construct a telescope which shall magnify a given number of times, say 100, we must employ two convex lenses, the focal distance of the one of which shall be 100 times greater than that of the other; in this case the one will give the object-glass, and the other the eye-glass. These must afterward be fitted on the same axis, so that their distance shall be equal to the sum of the two focal distances; that is, they must be fixed in a tube of this length, and then the eye being placed behind the eye-glass, at its focal distance, will see objects magnified 100 times.

This arrangement may be varied without end, by assuming an eye-glass at pleasure, and adapting to it an object-glass whose focal distance shall be 100 times greater. Thus, taking an eye-glass of one inch focus, the object-glass must be of 100 inches focus, and the distance of the lenses 101 inches. Or, taking an eye-glass of 2 inches focus, the object-glass must have its focus at the distance of 200 inches, and the distance of the lenses will be 202 inches. If you were to take an eye-glass of 3 inches focus, the focal distance of the object-glass must be 300 inches, and the distance of the lenses from each other 303 inches. And if you were to take an eye-glass of 4 inches focus, the object-glass must have a focal distance of 400 inches, and the distance of the two lenses 404 inches, and so on, the instrument always increasing in length. If, on the contrary, you were to assume an eye-glass of only half an inch focus, the object-glass must have a focal distance of 100 half-inches, that is, of 50 inches, and the distance between the lenses would only be 50 inches and a half, which is little more than four feet. And if an eye-glass of a quarter of an inch focus were to be employed, the object-glass would require a focal distance of only 100 quarters of an inch, or 25 inches, and the distance between the two lenses 25 inches and a quarter, that is little more than two feet.

Here, then, are several methods of producing the same effect, that of magnifying 100 times; and if every thing else were equal, we should not hesitate about giving the preference to the last, as being the shortest: for here the telescope. being reduced to little more than two feet, would be more manageable than one much longer.

No one, then, would hesitate about preferring the

shortest telescopes, provided all other circumstances were the same, and all the different species represented objects in the same degree of perfection. But though they all possess the same magnifying power, the representation is by no means equally clear and distinct. That of two feet in length certainly magnifies 100 times, as well as the others; but on looking through such a telescope, objects will appear not only dark, but blunt and confused, which is undoubtedly a very great defect. The last telescope but one, whose object-glass is 50 inches focus, is less subject to these defects: but the dimness and confusion are still insupportable; and these defects diminish in proportion as we employ greater object-glasses, and are reduced to almost nothing on employing an object-glass of 300 inches, with an eye-glass of 3 inches focus. On increasing these measurements, the representation becomes still clearer and more distinct; so that in this respect long telescopes are preferable to short, though otherwise less commodious. This circumstance imposes on me a new task, that of further explaining two very essential articles in the theory of telescopes: the one respects the clearness, or degree of light in which objects are seen; and the other the distinctness and accuracy of expression with which they are represented. Without these two qualities, all magnifying power, however great, procures no advantage for the contemplation of objects.

27th February, 1762.

LETTER XCVI.

Degree of Clearness.

In order to form a judgment of the degree of clearness in which objects are represented by the telescope, I shall recur to the same principles which I endeavoured to elucidate in treating the same subject with reference to the microscope.

And, first, it must be considered, that in this research it is not proposed to determine the degree of light resident in objects themselves, and which may be very different, not only in different bodies, as being in their nature more or less luminous, but in the same body, according as circumstances vary. The same bodies, when illuminated by the sun, have undoubtedly more light than when the sky is overcast, and in the night their light is wholly extinguished; but different bodies illuminated may differ greatly in point of brightness, according as their colours are more or less lively. We are not inquiring, then, into that light or brightness which resides in objects themselves; but, be it strong or faint, we say that a telescope represents the object in perfect clearness, when it is seen through the instrument as clearly as by the naked eye; so that if the object be dim, we are not to expect that the telescope should represent it as clear.

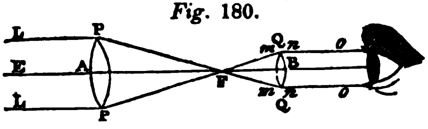
Accordingly, in respect of clearness, a telescope is perfect when it represents the object as clearly as it appears to the naked eye. This takes place, as in the microscope, when the whole opening of the pupil is filled with the rays which proceed from every point of the object, after being transmitted through the telescope. If a telescope furnishes rays sufficient to fill the whole opening of the pupil, no greater degree of clearness need be desired; and supposing it could supply rays in greater profusion, this would be entirely useless, as the same quantity precisely, and no more, could find admission into the eye.

Here, then, attention must be paid chiefly to the aperture of the pupil, which, being variable, prevents our laying down a fixed rule, unless we regulate ourselves according to a certain given aperture, which is sufficient, when the pupil, in a state of the greatest contraction, is filled with rays; and for this purpose the diameter of the pupil is usually supposed to be one line, twelve of which make an inch; we sometimes satisfy ourselves with even the half of this, allowing to the diameter of the pupil only half a line, and in some cases still less.

If you will please to consider that the light of the sun exceeds that of the moon 200,000 times, though even that of the moon is by no means inconsiderable, you will be sensible that a small diminution in point of clearness can be of no great consequence in the contemplation of objects. Having premised this, all that remains is to examine the rays which the telescope transmits into the eye, and to compare them with the pupil; and it will be sufficient to consider the rays which proceed from a single point of the object, that, for example, which is in the axis of the telescope.

1. The object being infinitely distant, the rays which fall from it on the surface of the object-glass P A P, *Fig.* 180, are parallel to each other: all the rays, then, which come from the centre of the object will be contained within the lines L P, L P, parallel to the axis E A. All these rays taken together are denominated the *pencil* of rays which fall on the object-glass, and the breadth of this pencil is equal to the extent or aperture of the object-glass, the diameter of which is P A P.

Fig. 180.



2. This pencil of rays is changed by the refraction of the object-glass into a conical or pointed figure P F P, and having crossed at the focus F, it

forms a new cone *m* F *m*, terminated by the eye-glass; hence it is evident that the base of this cone *m m* is as many times smaller than the breadth of the pencil P P, as the distance F B is shorter than the distance A F.

3. Now these rays F *m*, F *m*, on passing through the eye-glass Q B Q, become again parallel to each other, and form the pencil of rays *n o*, *n o*, which enter into the eye, and there depict the image of the point of the object whence they originally proceeded.

4. The question, then, resolves itself into the breadth of this pencil of rays *n o*, *n o*, which enter into the eye; for if this breadth *n n* or *o o* is equal to or greater than the opening of the pupil, it will be filled with them, and the eye will enjoy all possible clearness; that is, the object will seem as clear as it you were to look at it with the unassisted eye.

5. But if this pencil *n n*, *o o* were of much less breadth than the diameter of the pupil, it is evident that the representation must become so much more obscure; which would be a great defect in the telescope. In order to remedy it, the pencil must therefore be at least half a line in breadth; and it would be still better to have it a whole line in breadth, this being the usual aperture of the pupil.

6. It is evident that the breadth of this second pencil has a certain relation to that of the first, which it is very easy to determine. You have only to settle how many times the distance *n n* or *m m* is less than the distance P P, which is the aperture of the object-glass. But the distance P P is in the same proportion to the distance *m m*, as the distance A F to the distance B F, on which the magnifying power depends; accordingly, the magnifying power itself discovers how many times the pencil L P, L P is broader than the pencil *n o*, *n o*, which enters into the eye.

7. Since, then, the breadth *n n* or *o o* must be one line, at least half a line, the aperture of the object-glass P P must at least contain as many half-lines as the magnifying power indicates; thus, when the telescope is to magnify 100 times, the aperture of its object-glass must have a diameter of 100 half-lines, or 50 lines, which make 4 inches and 2 lines.

8. You see, then, that in order to avoid obscurity, the aperture of the object-glass must be greater in proportion as the magnifying power is greater. And, consequently, if the object-glass employed is not susceptible of such an aperture, the telescope will be defective in respect of clearness of representation.

Hence it is abundantly evident, that in order to magnify very greatly it is impossible to employ small object-glasses, whose focal distance is too short, as a lens formed by the arches of small circles cannot have a great aperture.

1st March, 1762.

LETTER XCVII.

Aperture of Object-glasses.

You have now seen that the magnifying power determines the size or extent of the object-glass, in order that objects may appear with a sufficient degree of clearness. This determination respects only the size or aperture of the object-glass; however, the focal distance is affected by it likewise, for the larger the lens is the greater must be its focal distance.

The reason of this is evident, as in order to form a lens whose focal distance is, for example, two inches, its two surfaces must be arches of a circle whose radius is likewise about two inches. I have therefore

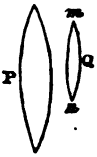


Fig. 181.

represented, *Fig.* 181, two lenses P and Q, the arches of which are described with a radius of two inches. The lens P, being the thicker, is much greater than the lens Q; but I shall demonstrate afterward that thick lenses are subject to other inconveniences, and these so great as to oblige us to lay them altogether aside. The lens Q, then, will be found more adapted for use, being composed of smaller arches of the same circle; and as its focal distance is two inches, its extent or aperture *m n* may scarcely exceed one inch. Hence this may be laid down as a general rule, that the focal distance of a lens must always be twice greater than the diameter of its aperture *m n*; that is, the aperture of a lens must of necessity be smaller than half the focal distance.

Having remarked, then, that in order to magnify 100 times, the aperture of the object-glass must exceed 4 inches, it follows that the focal distance must exceed 8 inches; I shall presently demonstrate that the double is not sufficient, and that the focal distance of this lens must be increased beyond 300 inches. The distinctness of the expression of the image requires this great increase, as shall afterward be shown: I satisfy myself with remarking, at present, that with regard to the geometrical figure of the lens, the aperture cannot be greater than half its focal distance.

Here, therefore, I shall go somewhat more into the detail respecting the aperture of the object-glass, which every magnifying power requires; and I remark, first, that though a sufficient degree of clearness requires an aperture of four inches, when the telescope is to magnify 100 times, we satisfy ourselves, in astronomical instruments, with one of three inches, the diminution of clearness being scarcely perceptible. Hence artists have laid it down as a rule, that in order to magnify 100 times, the aperture of the object-glass must be three inches; and for other magnifying powers in that proportion. Thus, in order to magnify 50 times, it is sufficient that the aperture of the object-glass be an inch and a half; to magnify 25 times, three-quarters of an inch suffice, and so of other powers.

Hence we see that for small magnifying powers a very small aperture of the object-glass is sufficient, and that, consequently, a moderate focal distance may answer. But if you wished to magnify 200 times, the aperture of the object-glass must be six inches, or half a foot, which requires a very large lens, whose focal distance must exceed even 100 feet, in order to obtain a distinct and exact expression. For this reason, great magnifying powers require very long telescopes, at least according to the usual arrangement of lenses which I have explained. But for some time past artists have been successfully employing themselves in diminishing this excessive length. The aperture of the object-glass, however, must follow the rule laid down, as clearness necessarily depends on it.

Were you desirous, therefore, of constructing a telescope which should magnify 400 times, the aperture of the object-glass must be twelve inches, or a foot, let the focal distance be rendered as small as you will: and if you wished to magnify 4000 times, the aperture of the object-glass must be ten feet,—a very great size indeed, and too much so for any artist to execute; and this is the principal reason why we can never hope to carry the magnifying power so far, unless some great prince would be at the expense of providing and executing lenses of such magnitude; and, after all, perhaps they would not succeed.

A telescope, however, which should magnify 4000 times, would discover many wonderful things in the heavens. The moon would appear 4000 times larger than to the naked eye; in other words, we should see her as if she were 4000 times nearer to us than she is. Let us inquire, then, to what a degree we might be able to distinguish the different bodies which she may contain. The distance of the moon from the earth is calculated to be 240,000 English miles, the 4000dth part of which is 60 miles: such a telescope would accordingly show us the moon as if she were only 60 miles distant; and, consequently, we should be enabled to discover in her the same things which we distinguish in objects removed to the same distance. Now, from the top of a mountain we can easily discern other mountains more than 60 miles distant. There can be no doubt, then, that with such an instrument we should discover on the surface of the moon many things to fill us with surprise. But in order to determine whether the moon is inhabited by creatures similar to those of the earth, a distance of 60 miles is still too great; we must have, in order to this effect, a telescope which should magnify ten times more, that is 40,000 times, and this would require an object-glass of 100 feet aperture, an enterprise which human art will never be able to execute. But with such an instrument we should see the moon as if she were no farther distant than from Berlin to Spandau, and good eyes might easily discern men at this distance, if any there were, but too indistinctly, it must be allowed, to be completely assured of the fact.

As we must rest satisfied with wishing on this subject, mine should be to have at once a telescope which should magnify 100,000 times;* the moon would then appear as if she were only half a mile distant.

The aperture of the object-glass of the telescope must be 250 feet, and we should see, at least, the larger animals which may be in the moon.

6th March, 1762.

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* Dr. Herschel has been able to apply a magnifying power of 6500 times to the fixed stars.—*Ed.*

LETTER XCVIII.

On Distinctness in the Expression: On the Space of Diffusion occasioned by the Aperture of Object-glasses, and considered as the first Source of Want of Distinctness in the Representation.

Distinctness of expression is a quality of so much importance in the construction of telescopes, that it seems to take precedence of all the others which I have been endeavouring to explain; for it must be allowed that a telescope which does not represent distinctly the images of objects must be very defective. I must therefore unfold the reasons of this want of distinctness, that we may apply more successfully to the means of remedying it.

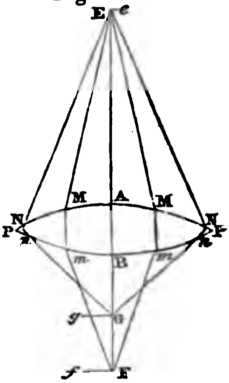
They appear so much the more abstruse, that the principles hitherto laid down do not discover the source: in fact, this defect is thus to be accounted for—one of the principles on which I have hitherto proceeded is not strictly true, though not far from the truth.

You will recollect that it has been laid down as a principle, that a convex lens collects into one point of the image all the rays which come from one point of the object. Were this strictly true, images represented by lenses would be as distinctly expressed as the object itself, and we should be under no apprehension of defect in regard to this.

Here, then, lies the defectiveness of this principle; lenses have the property now ascribed to them only around their centre; the rays which pass through the extremities of a lens collect in a different point from those which pass towards the centre, though all proceed from the same point of the object; hence are produced two different images, which occasion indistinctness.

In order to set this in the clearest light, let us consider the convex lens P P, *Fig.* 182, on the axis of which is placed the object E *e*, of which the point E, situated upon the axis, emits the rays E N, E M, E A, E M, E N, to the surface of the lens. To the direction of these rays, as changed by refraction, we must now pay attention.

Fig. 182.



1. The ray E A, which passes through the centre A of the lens, undergoes no refraction, but proceeds forward in the same direction, on the straight line A B F.

2. The rays E M and E M, which are nearest to the first, undergo a small refraction, by which they will meet with the axis somewhere at F, which is the place of the image F *f*, as has been explained in some of my preceding Letters on this subject.

3. The rays E N and E N, which are more remote from the axis E A, and which pass towards the extremities N N of the lens, undergo a refraction somewhat different, which collects them, not at the point F, but at another point G, nearer the lens: and these rays represent another image G *g*, different from the first F *f*.

4. Let us now carefully attend to this particular circumstance, not hitherto remarked: it is this, that the rays passing through the lens, towards its extremities, represent another image G *g*, than what is represented by those passing near the centre M A M.

5. If the rays E N, E N, were to retire still farther from the centre A, and to pass through the points P P, of the lens, their point of reunion would be still

nearer to the lens, and would form a new image, nearer than even G *g*.

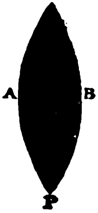
6. Hence you will easily perceive, that the first image F *f*, which is named the principal image, is formed only by the rays which are almost infinitely near the centre; and that according as the rays retire from it, towards the extremities of the lens, a particular image is formed nearer the lens, till those passing close to the extremities form the last, G *g*.

7 All the rays, therefore, which pass through the lens P P represent an infinity of images disposed between F *f* and G *g*; and at every distance from the axis the refraction of the lens produces a particular image, so that the whole space between F and G is filled with a series of images.

8. This series of images is accordingly denominated the diffusion of the image; and when all these rays afterward enter into an eye, it is natural that the vision should be so much disturbed as the space F G, through which the image is diffused, is more considerable. If this space F G could be reduced to nothing, no confusion need be apprehended.

9. The greater portions of their respective circles that the arches P A P and P B P are, the greater likewise is F G the space of diffusion. You see a good reason, then, for rejecting all lenses of too great thickness, or in which the arches which form the surfaces of the lens are considerable segments of their circles, as in *Fig.* 183, of which the arches P A P and P B P are the fourth part of the whole circumference, so that each contains 90°; this would, consequently produce an insufferable confusion.

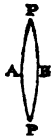
Fig. 183.



10. The arches, then, which form the surfaces of a lens, must contain much less than 90 degrees: if they contained so much as 60, the diffusion of the image would be even then insupportable. Authors who have treated the subject admit

of 30 degrees at most: and some fix the boundary at 20 degrees. A lens of this last description is represented by *Fig.* 184, in which the arches P A P and P B P contain only 20 degrees, each being but the eighteenth part of the whole circumference of its respective circle.

Fig. 184.



11. But if this lens were to supply the place of the object-glass in a telescope, the arches P A P and P B P must contain still many degrees less. For though the diffusion of the image be perceptible of itself, the magnifying power multiplies it as many times as it does the object. Therefore, the greater the magnifying power proposed, the fewer must be the number of degrees which the surfaces of the lens contain.

12. When the telescope is intended to magnify 100 times, you will recollect that the aperture of the object-glass must be 3 inches, and its focal distance 360 inches, which is equal to the radii with which the two arches P A P and P B P are described; hence it follows that each of these two arches contains but half a degree; and it is distinctness of expression which requires an arch so small. If it were intended to magnify 200 times, half a degree would be still too much, and the measure of the arch, in that case, ought not to exceed the third part of a degree. This arch, however, must receive an extent of 6 inches; the radius of the circle must therefore be so much greater, and consequently also the focal distance. This is the true reason why great magnifying powers require telescopes of such considerable length.

9th March, 1762.

LETTER XCIX.

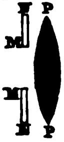
Diminution of the Aperture of Lenses, and other means of lessening the Space of Diffusion till it is reduced to nothing.

When the space of an object-glass is too great to admit of distinctness of expression, it may be very easily remedied: you have only to cover the lens with a circle of pasteboard, leaving an opening in the centre, so that the lens may transmit no other rays but those which fall upon it through the opening, and that those which before passed through the extremities of the lens may be excluded; for as no rays are transmitted but through the middle of the lens, the smaller the opening is the smaller likewise will be the space of diffusion. Accordingly, by a gradual diminution of the opening, the space of diffusion may be reduced at pleasure.

Here the case is the same as if the lens were no larger than the opening in the pasteboard, thus the covered part becomes useless, and the opening determines the size of the lens; this then is the remedy employed to give object-glasses any given extent.

P P is the object-glass, *Fig.* 185, before which is placed the pasteboard N N, having the opening M M, which is now the extent of the lens. This opening M M is here nearly the half of what it would be were the pasteboard removed; the space of diffusion is therefore much smaller. It is remarked, that the space of diffusion in this case is only the fourth part of what it was before. An opening M M, reduced to a third of P P, would render the space of diffusion nine times less. Thus the effect of this remedy is very considerable; and on covering the extremities of

Fig. 185.



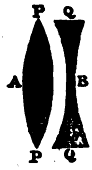
the lens ever so little, the effect of it becomes perceptible.

If, therefore, a telescope labours under this defect, that it does not represent objects sufficiently distinct, as a series of images blended together must of necessity produce confusion, you have only to contract the aperture of the object-glass by a covering of pasteboard such as I have described, and this confusion will infallibly disappear. But a defect equally embarrassing is the consequence; the degree of brightness is diminished. You will recollect that every degree of the magnifying power requires a certain aperture of the object-glass, that as many rays may be transmitted as are necessary to procure a sufficient illumination. It is vexatious, therefore, in curing one defect, to fall into another; and in order to the construction of a very good telescope, it is absolutely necessary that there should be sufficient brightness of illumination, without injuring distinctness in the representation.

But can there be no method of diminishing, nay, of totally reducing the space of diffusion of object-glasses without diminishing the aperture? This is the great inquiry which has for some time past engaged the attention of the ingenious, and the solution of which promises such a field of discovery in the science of dioptrics. I shall have the honour, at least, of laying before you the means which scientific men have suggested for this purpose.

As the focus of the rays which pass through the middle of a convex lens is more distant from the lens than the focus of the rays which pass through the extremities, it has been remarked that concave lenses produce a contrary effect. This has suggested the inquiry, whether it might not be possible to combine a convex with a concave lens, in such a manner that the space of diffusion should be entirely annihilated; while, in other respects, this compound lens should produce the same effect as an ordinary simple object-glass? You know that concave lenses are measured by their focal distance as well as those which are convex; with this difference, that the focus of the concave is only imaginary, and falls before the lens, whereas the focus of convex lenses is real, and falls behind them. Having made this remark, we reason as follows:

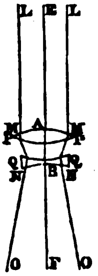
Fig. 186.



1. If we place, *Fig.* 186, behind a convex lens P A P, a concave one Q B Q of the same focal distance, the rays which the convex lens would collect in its focus will be refracted by the concave, so that they will again become parallel to each other, as they were before passing through the convex lens.

2. In this case, therefore, the concave lens destroys the effect of the convex, and it is the same thing as if the rays had proceeded in their natural direction, without undergoing any refraction. For the concave lens, having its focus at the same point F (see *Fig.* 178, p. 331), restores the parallelism of the rays, which would otherwise have met at the point F.

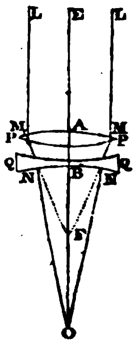
Fig. 187.



3. If the focal distance of the concave lens were smaller than that of the convex, it would produce a greater effect, and would render the rays divergent, as in *Fig.* 187: the incident parallel rays L M, E A, L M, passing through the two lenses, would assume the directions N O, B F, N O, which are divergent from each other. These two lenses together produce, therefore, the same effect as a simple concave lens, which would impress on the incident parallel rays the same divergence. Two such lenses joined together, of which the concave has a smaller focal distance than the convex, are therefore equivalent to a simple concave lens.

4. But if the concave lens Q Q, *Fig.* 188, has a greater focal distance than the convex lens P P, it is not even sufficient to render parallel to each other the rays which the convex lens by itself would collect in its focus F: these rays, therefore, continue convergent, but their convergence will be diminished by the concave lens, so that the rays, instead of meeting in the point F, will meet in the more distant point O.

Fig. 188



5. These two lenses joined together will produce, then, the same effect as a simple convex lens which should have its focus at O, as it would collect the parallel rays L M, E A, L M, equally in the same point. It is therefore evident that two lenses may be combined an infinite variety of ways, the one being convex and the other concave, so that their combination shall be equivalent to a given convex lens.

6. Such a double object-glass may therefore be employed in the construction of telescopes, instead of the simple one, to which it is equivalent; and the effect as to the magnifying power will be just the same. But the space of diffusion will be quite different, and it may happen to be greater or less than that of a simple object-glass; and in this last case the double object-glass will be greatly preferable to the simple one.

7. But, further, it has been found possible to arrange two such lenses so that the space of diffusion is reduced absolutely to nothing, which is undoubtedly the greatest advantage possible in the construction of telescopes. Calculation enables us to determine this arrangement, but no artist has hitherto been found capable of reducing it to practice.

13th March, 1762.

LETTER C.

Of Compound Object-glasses.

The combination of two lenses, of which I have now given the idea, is denominated a compound object-glass; the end proposed from them is, that all the rays, as well those which pass through the extremities of a lens as those which pass through the middle, should be collected in a single point, so that only one image may be formed, without diffusion, as in simple object-glasses. Could artists succeed in effecting such a construction, very great advantages would result from it, as you shall see.

It is evident, first, that the representation of objects must be much more distinct, and more exactly expressed, as vision is not disturbed by the apparition of that series of images which occupy the space of diffusion when the object-glass is simple.

Again, as this space of diffusion is the only reason which obliges us to give to simple object-glasses such an excessive focal distance, in order to render the inconvenience resulting from it imperceptible, by employing compound object-glasses we are relieved from that cumbersome expedient, and are enabled to construct telescopes incomparably shorter, yet possessing the same magnifying power.

When, employing a single object-glass, you want to magnify a hundred times, the focal distance cannot be less than thirty feet, and the length of the telescope becomes still greater on account of the eye-glass, whose focal distance must be added; a small object-glass would produce, from its greater space of diffusion, an intolerable confusion. But a length of thirty feet is not only very incommodious, but artists seldom succeed in forming lenses of so great a focal distance. You will readily perceive

the reason of this; for the radius of the surfaces of such a lens must likewise be thirty feet, and it is very difficult to describe exactly so great a circle, and the slightest aberration renders all the labour useless.

Accidents of this sort are not to be apprehended in the construction of compound object-glasses, which may be formed of smaller circles, provided they are susceptible of the aperture which the magnifying power requires. Thus, in order to magnify one hundred times, we have seen that the aperture of the object-glass must be three inches; but it would be easy to construct a compound object-glass whose focal distance should be only one hundred inches, and which could admit an aperture of more than three inches: therefore, as the focal distance of the eye-glass must be one hundred times smaller, it would be one inch; and the interval between the lenses being the sum of their focal distances, the length of the telescope would be only one hundred and one inches, or eight feet five inches, which is far short of thirty feet.

But it appears to me that a compound object-glass, whose focal distance should be fifty inches, might easily admit an aperture of three inches, and even more: taking, then, an eye-glass of half an inch focus, you will obtain the same magnifying power of one hundred times, and the length of the telescope will be reduced one-half, that is, to four feet and less than three inches. Such a telescope, then, would produce the same effect as a common one of thirty feet, which is assuredly carrying it as far as need be wished.

If such a compound object-glass could be made to answer, you would only have to double all these measurements in order to have one which should admit an aperture of six inches; and this might be employed to magnify two hundred times, making use of an eye glass of half an inch focus as the two hundredth part of the focal distance of the object-glass, which would, in this case, be one hundred inches. Now, a common telescope which should magnify two hundred times, must exceed one hundred feet in length; whereas this one, which is constructed with a compound object-glass, is reduced to about eight feet, and is perfectly accommodated to use, whereas a telescope of one hundred feet long would be an unwieldly and almost useless load.

The subject might be carried still much further, and by again doubling the measurements, we might have a compound object-glass whose focal distance should be two hundred inches, or sixteen feet eight inches, which should admit of an aperture of twelve inches, or one foot: taking, then, an eye-glass of half an inch focus, as two hundred inches contain four hundred half-inches, we should have a telescope capable of magnifying four hundred times, and still abundantly manageable, being under seventeen feet; whereas, were we to attempt to produce the same magnifying power with a simple object-glass, the length of the telescope must exceed three hundred feet, and consequently could be of no manner of use on account of that enormous size.

They have at Paris a telescope one hundred and twenty feet long, and one at London of one hundred and thirty feet; but the dreadful trouble of mounting and pointing them to the object almost annihilates the advantages expected from them. From this you will conclude of what importance it would be to succeed in the construction of the compound lenses which I have been describing. I suggested the first idea of them several years ago, and since then artists of the greatest ability in England and France have been attempting to execute them. Repeated efforts and singular skill in the artist are undoubtedly requisite. Indeed, I have made, with the assistance of an able mechanician of our Academy, some not unsuccessful attempts; but the expense attending such an enterprise has obliged me to give it up.

But the Royal Society of London last year announced, that an eminent artist, of the name of *Dollond*,* had fortunately succeeded; and his telescopes are now universally admired. An able artist of Paris, named *Passement*, boasts of a similar success. Both these gentlemen did me the honour, some time ago, to correspond with me on the subject; but as the point in question was chiefly how to surmount certain great difficulties in the practical part, which I never attempted, it is but fair that I should relinquish to them the honour of the discovery. The theory alone is my province, and it has cost me much profound research, and many painful calculations, the very sight of which would terrify you. I shall therefore take care not to perplex you further with this abstruse inquiry.

16th March, 1762.

LETTER CI

Formation of Simple Object-glasses.

In order to give you some idea of the researches which led me to the construction of compound object-glasses, I must begin with the formation of the simple lens.

Observe, first, that the two surfaces of a lens may be formed in an infinity of different ways, by taking circles of which the surfaces are segments, either equal or unequal to each other, the focal distance, however, remaining always the same.

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* The first achromatic telescope ever constructed was made by *Chester More Hall*, Esq., of More-hall, in Essex, in the year 1733, no less than *twenty-four* years before the period alluded to by our author. This invaluable instrument, is, therefore, in every view of the matter, a British invention. See the article *Optics*, in the *Edinburgh Encyclopædia*, vol. xv. p. 479, note, for a full account of *Mr. Hall's* labours.—*Ed.*

The same figure is usually given to both surfaces of a lens, or, as the surfaces of a lens are represented by arches of a circle, both surfaces are formed with radii equal to each other. Facility of execution has undoubtedly recommended this figure, as the same basin serves to form both surfaces, and most artists are provided with but few basins.

Suppose, then, a convex lens, both whose surfaces are polished on the same basin, one of twenty-four inches radius, so that each surface shall be an arch of the circle whose radius is twenty-four inches: this lens will be convex on both sides, and will have its focal distance at twenty-four inches, according to the common calculation; but as the focus depends on the refraction, and as the refraction is not absolutely the same in every species of glass, in which we find a very considerable diversity, according as the glass is more or less white and hard, this calculation of the focus is not strictly accurate; and usually the focal distance of the lens is somewhat less than the radius of its two surfaces, sometimes the tenth part, sometimes the twelfth: accordingly, the supposed lens, the radius of whose surfaces is twenty-four inches, will have its focus at the distance of about twenty-two inches, if it is formed of the same species of glass of which mirrors are commonly manufactured; though even in glass of this sort we meet with a small diversity in respect of refraction.

We see afterward, that on making the two surfaces of the lens unequal, an infinity of other lenses may be formed, which shall all have the same focal distance; for on taking the radius of one of the surfaces less than twenty-four inches, that of the other surface must be taken greater in proportion, according to a certain rule. The radius of one of the surfaces may always be taken at pleasure; and by means of a certain rule the radius of the other may be found, in order that the focal distance may become the same as if each surface had been formed on a radius of twenty-four inches. The following table exhibits several such lenses, which have all the same focal distance.

Lenses.	*Radius of the first Surface.*	*Radius of the Second Surface.*
I.	24	24
II.	21	28
III.	20	30
IV.	18	36
V.	16	48
VI.	15	60
VII.	14	84
VIII.	13	156
IX.	12	infinity

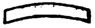
In the last form, the radius of one surface is only 12 inches, or the half of 24 inches; but that of the other becomes infinite: or rather, this surface is an arch of a circle infinitely great; and as such an arch differs nothing from a straight line, this may be considered as a plane surface, and such a lens is plano-convex.

Were we to assume the radius of a surface still smaller than 12 inches, the other surface must be made concave, and the lens will become convexo-concave; it will, in that case, bear the name of *meniscus*, several figures of which are represented in the following table:—

Meniscus.	*Radius of the Convex Surface.*	*Radius of the Concave Surface.*
X.	11	132
XI.	10	60
XII.	9	36
XIII.	8	24
XIV.	6	12
XV.	4	6
XVI.	3	4

Here, then, is a new species of lenses, the last of which is represented in *Fig.* 190, so that we have now sixteen different species, which have all the same focal distance; and this is about 22 inches, a little more or less, according to the nature of the glass.

Fig. 190.



When, therefore, the only question is, What focal distance the lens ought to have? it is a matter of indifference according to which of these forms you go to work; but there may be a very great difference in the space of diffusion to which each species is subjected, this space becoming smaller in some than in others. When a simple object-glass is to be employed, as is usually done, it is by no means indifferent of what figure you assume it, for that which produces the smallest space of diffusion is to be preferred. Now, this excellent property does not belong to the first species, where the two surfaces are equal; but nearly to species VII., which possesses the quality, that when you turn towards the object its more convex surface, or that whose radius is smallest, the space of diffusion is found to be about one-half less than when the lens is equally convex on both sides: this, therefore, is the most advantageous figure for simple object-glasses, and practitioners are accordingly agreed in the use of it.

It is evident, then, that in order to ascertain the space of diffusion of a lens, it is not sufficient to know its focal distance; its species likewise must be determined, that is, the radii of each surface; and you must carefully distinguish which side is turned to the object.

After this explanation, it is necessary to remark, that in order to discover the combination of two lenses which shall produce no diffusion of image, it is absolutely necessary to take into the account the figure of both surfaces of each glass, and to resolve the following problem: *What must be the radii of the surfaces of two lenses, in order to reduce to nothing the space of diffusion?* The solution requires the most profound researches of the most sublime geometry; and supposing these to have been successful, the artist has, after all, many difficulties to surmount. The basins must have precisely that curve which the calculation indicates; nor is that sufficient, for in the operation of forming the lens on the basin, the basin suffers from the friction in its turn; hence it becomes necessary to rectify its figure from time to time, with all possible accuracy, for if all these precautions are not strictly observed, it is impossible to ensure success; and it is no easy matter to prevent the lens from assuming a figure somewhat different from that of the basin in which it is moulded. You must be sensible, from all this, how difficult it must be to carry to perfection this important article in dioptrics.

20*th March*, 1762.

LETTER CII.

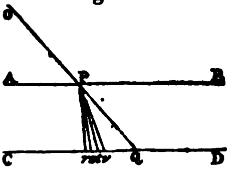
Second Source of Defect as to Distinctness of Representation by the Telescope. Different Refrangibility of Rays.

You have now seen in what manner it may be possible to remedy that defect in lenses which arises from the different refraction of rays, as those which pass through the extremities of a lens do not meet in the same point with those which pass through its middle, the effect of which is an infinity of images dispersed through the space of diffusion. But this is not the only defect; there is another, of so much more importance that it seems impossible to apply a remedy, as the cause exists, not in the glass, but in the nature of the rays themselves.

You will recollect that there is a great variety in rays, with respect to the different colours of which they convey the impression. I have compared this diversity to that which we meet with in musical notes, having laid it down as a principle, that each colour is attached to a certain number of vibrations. But supposing that this explanation should still appear doubtful, it is beyond all doubt that rays of different colours likewise undergo different refractions in their passage from one transparent medium to another; thus, red rays undergo the least refraction, and violet the greatest, though the difference is almost imperceptible. Now, all the other colours, as orange, yellow, green, and blue, are contained, with respect to refraction, within these two limits. It must likewise be remarked, that white is a mixture of all the colours which by refraction are separated from each other.

In fact, when a white ray O P, *Fig*. 191, or a ray of the sun, falls obliquely on a piece of glass A B C D, instead of pursuing its course in the direction P Q, it not only deviates from this, but divides into a variety of rays, P *r*, P *s*, P *t*, P *v*: the first of which P *r*, the one that deviates least, represents the red colour, and the last P *v*, which deviates most, the violet colour. The dispersion *r v* is indeed much smaller than it appears in the figure; the divergence, however, always becomes more perceptible.

Fig. 191.



From this different refrangibility of rays, according to their different colours, are produced the following phenomena with respect to dioptric glasses:

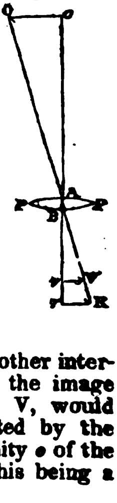
1. Let P P, *Fig*. 192, be a convex lens, on the axis of which O R, at a very great distance A O, is the object O *o*, the image of which, as represented by the lens, we are to determine, putting aside here the first irregularity, that which respects diffusion; or, which amounts to the same thing, attending

to those rays only which pass through the centre of the lens A B, as if its extremities were covered with a circle of pasteboard.

2. Let us now suppose the object O *o* to be red, so that all its rays shall be of the same nature; the lens will somewhere represent the image of it R *r* equally red; the point R is, in this case, denominated the focus of the red rays, or of those which undergo the least refraction.

3. But if the object O *o* is violet, as rays of this colour undergo the greatest refraction, the image V *v* will be nearer the lens than R *r*; this point *v* is called the focus of violet rays.

Fig. 192.



4. If the object were painted some other intermediate colour between red and violet, the image would fall between the points R and V, would be always very distinct, and terminated by the straight line *o* B, drawn from the extremity *o* of the object, through the centre of the lens, this being a general rule for all colours.

5. But if the colour of the object is not pure, as is the case in almost all bodies, or if the object is white, which is a mixture of all colours, the different species of rays will then be separated by refraction, and each will represent an image apart. That which is formed of red rays will be at R *r*; and that which is produced by the violet at V *v*; and the whole space R V will be filled with images of the intermediate colours.

6. The lens P P, then, will represent a succession of images of the object O *o*, disposed through the small space R V, of which the most remote from the lens is red, and the nearest V *v* violet, and the intermediate images of the intermediate colours,

according to the order of the colours as they appear in the rainbow.

7. Each of these images will be abundantly distinct in itself, and all terminated by the straight line *o* B *v r*, drawn from the extremity *o* of the object through the centre of the lens B; but they could not be viewed together without a very perceptible confusion.

8. Hence, then, is produced a new space of diffusion, as in the first irregularity; but differing from it in this—that the latter is independent on the aperture of the lens, and that each image is painted of a particular colour.

9. This space of diffusion R V depends on the focal distance of the lens, so as to be always about the 28th part; when, therefore, the focal distance of the lens P P is 28 feet, the space R V becomes equal to an entire foot, that is, the distance between the red image R *r* and the violet V *v* is one foot. If the focal distance were twice as great, or 56 feet, the space R V would be two feet; and so of other distances.

10. Hence the calculation of the focal distance of a lens becomes uncertain, as the rays of each colour have their separate focus; when, therefore, the focus of a lens is mentioned, it is always necessary to announce the colour that we mean. But rays of an intermediate nature are commonly understood, those between red and violet, namely the green.

11. Thus, when it is said, without further explanation, that the focal distance of such a lens is 56 feet, we are to understand that it is the green image which falls at that distance; the red image will fall about a foot farther off, and the violet a foot nearer.

Here, then, is a new circumstance of essential importance, to which attention must be paid in the construction of dioptrical instruments.

23d March, 1762.

LETTER CIII.

Means of remedying this Defect by Compound Object-glasses.

It is necessary carefully to distinguish this new diffusion or multiplication of the image, arising from the different refrangibility of rays, as being of different colours from the first diffusion, occasioned by the aperture of the lens, inasmuch as the rays which pass through the extremities form another image than those which pass through its middle. This new defect must accordingly be remedied differently from the first.

You will please to recollect that I have proposed two methods for remedying the preceding defect; the one consisted in an increase of the focal distance, in order to diminish the curve of the surfaces of the lens. This remedy introduces instruments extremely long whenever a great magnifying power is required. The other consists in a combination of two lenses, the one convex and the other concave, to modify the refraction, so that all the rays transmitted through these lenses may meet in the same point, and the space of diffusion be totally reduced.

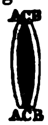
But neither of these remedies affords the least assistance towards removing the inconvenience arising from the different refrangibility of rays. The first even increases the evil; for the more that the focal distance is increased, the more considerable becomes the space through which the coloured images are dispersed. Neither does the combination of two or more lenses furnish any assistance; for we are assured, from both theory and experience, that the images of different colours remain always separated, however great the number of lenses through which the rays are transmitted, and that the more the lens magnifies, the more the difference increases.

This difficulty appeared so formidable to the great *Newton*, that he despaired of finding a remedy for a defect which he believed absolutely inseparable from dioptrical instruments, when the vision is produced by refracted rays. For this reason he resolved to give up refraction altogether, and to employ mirrors instead of object-glasses, as reflection is always the same for rays of every nature. This idea has procured for us those excellent reflecting telescopes, whose surprising effects are so justly admired, and which I shall describe after I have explained every thing relative to refractive instruments.

On being convinced that it was impossible to remedy the different refrangibility of rays by a combination of several lenses, I remarked that the reason of it was founded on the law of refraction, which is the same in every species of glasses; and I perceived that if it were possible to employ other transparent substances, whose refraction should be considerably different from that of glass, it might be very possible to combine such substance with glass, in such a manner that all the rays should unite in the formation of a single image, without any space of diffusion. In pursuance of this idea, I found means to compose object-glasses of glass and water, wholly exempt from the effect of the different refrangibility of rays, which consequently would produce as good an effect as mirrors.

I executed my idea with two meniscuses, or concavo-convex lenses, *Fig.* 193, the one of which is A A C C, and the other B B C C, which I joined together with the concave surfaces towards each other, filling the void between them with water, so that the rays which entered by the lens A A C C must pass through the water enclosed between the two lenses, before they went off

Fig. 193.



through C C B B. Each ray undergoes, then, four refractions: the first on passing from the air into the lens A A C C; the second on passing from this lens into the water; the third on passing thence into the other lens C C B B; the fourth on passing from this lens into the air.

As the four surfaces of these two lenses here enter into consideration, I found means to determine their semi-diameters, so that of whatever colour a ray of light might be, after having undergone these four refractions, it should reunite in the same point, and the different refrangibility no longer produce different images.

These object-glasses, compounded of two lenses and water, were found subject at first to the former defect, namely, that of the rays which pass through the extremities forming a different focus from what is formed by those which pass through the middle; but, after much painful research, I found means to proportion the radii of the four surfaces in such a manner that these compound object-glasses became wholly exempted from the defects of both the classes specified. But it was necessary, to this effect, to execute so exactly all the measurements prescribed by the calculation, that the slightest aberration must become fatal to the whole process; I was therefore obliged to abandon the construction of these object-glasses.*

Besides, this project could remedy only the inconveniences which affect the object-glass, and the eye-glass might still labour under some defect as great, which it would be impossible to remedy in the same manner. Several eye-glasses are frequently employed in the construction of telescopes, which I shall describe afterward: we should not, therefore, gain much by a too scrupulous adherence to the object-glass only, while we overlook the other lenses, though their effect may not be greatly perceptible relatively to that of the object-glass.

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* Object-glasses of this kind, even if executed in the most correct manner, are incapable of producing the effects which our author expected from them.—*Ed.*

But whatever pains these researches may have cost me, I frankly declare that I entirely give up at present the construction of object-glasses compounded of glasses and water; as well on account of the difficulty of execution, as that I have since discovered other means, not of destroying the effect of the different refrangibility of rays, but of rendering it imperceptible. This shall be the subject of my next Letter.

27*th March*, 1762.

LETTER CIV.

Other Means more practicable.

Since the reflecting telescope came into general use, refracting ones have been so run down that they are on the point of being wholly laid aside. The construction of them has accordingly for some time past been wholly suspended, under a firm persuasion that every effort to raise them to a state of perfection would be useless, as the great *Newton* had demonstrated that the insurmountable difficulties arising from the different refrangibility of rays was absolutely inseparable from the construction of telescopes.

If this sentiment be well founded, there is no telescope capable of representing objects but with a confusion insupportable in proportion to the greatness of the magnifying power. However, though there are telescopes extremely defective in this respect, we likewise meet with some that are excellent, and nowise inferior to the so much boasted reflecting telescopes. This is undoubtedly a very great paradox; for if this defect really attached to

the subject, we should not find a single exception. Such an exception, therefore—and we have the testimony of experience that it exists—well merits every degree of attention.

We are to inquire, then, how it happens that certain telescopes represent the object abundantly distinct, while others are but too much subject to the defect occasioned by the different refrangibility of rays. I think I have discovered the reason, which I submit in the following reflections:—

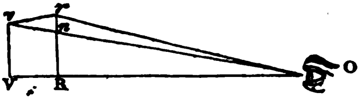
1. It is indubitably certain that the object-glass represents an infinity of images of each object, which are all arranged over the same space of diffusion, and each of which is painted its own proper colour, as I have demonstrated in the preceding Letter.

2. Each of these images becomes an object, with respect to the eye-glass, which represents each separately, in the colour proper to it; so that the eye discovers, through the telescope, an infinity of images, disposed in a certain order, according to the refraction of the lens.

3. And if, instead of one eye-glass, we were to employ several, the same thing will always take place, and instead of one image, the telescope will represent an infinity to the eye, or a series of images, each of which expresses a separate object, but of a particular colour.

4. Let us now consider, *Fig.* 194, the last images

Fig. 194.



presented by the telescope to an eye placed at O, and let R *r* be the red image, and V *v* the violet, those of the other colours being between these two, according to the order of their different refrangibility. I have not in this figure introduced the lenses of the telescope, the only point at present being to show in what manner the eye sees the images. Only we must conceive the distance of the eye O from these images to be very great.

5. All these images R *r* and V *v*, with the intermediate, are situated, then, on the axis of the telescope O R V, and terminated by a certain straight line, *r v*, denominated the terminatrix of all the images.

6. As I have represented these images in the figure, the red image R *r* is seen by the eye at O, under the angle R O *r*, which is greater than the angle V O *v*, under which the violet image V *v* is seen. The violet rays which, from the image V *v*, enter into the eye, are therefore blended with the red which come from the part R *r* of the red image R *r*.

7. Consequently, the eye cannot see the violet image without a mixture of rays of other colours, but which correspond to different points of the object itself; thus the point *n* of the red image is confounded in the eye with the extremity *v* of the violet image, from which a very great confusion must arise.

8. But the ray *r* O not being mixed with the others, the extremity seen will appear red, or the image will seem bordered with red, which afterward successively blends with these other colours, so that the object will appear with a party-coloured border; a fault very common in telescopes, to which some, however, are less subject than others.

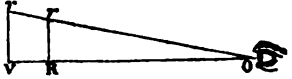
9. If the greater image R *r* were the violet, and V *v* the red, the confusion would be equally offensive, with this difference only, that the extremities of the object would then appear bordered with violet instead of red.

10. The confusion depends, then, on the position

of the terminating straight line *r v* with relation to the line V O, and the diversity which may take place in it; the result must be, that the confusion will be sometimes greater and sometimes less.

11. Let us now consider the case in which the last images represented by the telescope are so arranged, that the straight terminating line *v r*, being produced, would pass precisely into the eye. The eye will then see, *Fig.* 195, along a single ray *v r* O,

Fig. 195.



all the extremities; and, in general, all the points which correspond to one and the same point of the object will be conveyed to the eye by a single ray, and will there, consequently, be distinctly represented.

12. Here, then, is a case in which, notwithstanding the diversity of images, the eye may see the object distinctly, without any confusion of the different parts, as happened in the preceding case. This advantage, then, will be obtained when the terminating line *v r*, being produced, passes through the place of the eye O.

13. As the arrangement of the last images R *r* and V *v* depends on the disposition of the eye-glasses, in order to rescue telescopes from the defect imputed to them, nothing more is requisite but to arrange these lenses in such a manner that the terminating line of the last images *v r* shall pass through the eye; and telescopes thus constructed will always be excellent.

30*th March*, 1762.

LETTER CV.

Recapitulation of the Qualities of a good Telescope.

On taking a general review of the subject, you will readily admit that an excellent telescope is a most valuable commodity, but rarely to be met with, being subject to so many defects, and so many qualities being requisite, each of which has an essential influence on the construction of the instrument. As the number of the good qualities is considerable, in order that no one of them may escape your observation, I shall again go over the ground, and make a distinct enumeration of them.

1. The first respects the magnifying power; and the more that a telescope magnifies objects, the more perfect undoubtedly it is, provided that no other good quality is wanting. Now, the magnifying power is to be estimated from the number of times that the diameter of the object appears greater than to the naked eye. You will recollect that, in telescopes of two lenses, the magnifying power is so many times greater as the focal distance of the object-glass exceeds that of the eye-glass. In telescopes consisting of more lenses than two, the determination of the magnifying power is more intricate.

2. The second property of a good telescope is brightness. It is always very defective when it represents the object obscurely, and as through a mist. In order to avoid this defect, the object-glass must be of such a size as is regulated by the magnifying power. Artists have determined that, in order to magnify 300 times, the aperture of the object-glass ought to be three inches diameter; and for every other magnifying power in proportion. And when objects are not very luminous of themselves, it

would be proper to employ object-glasses of a still greater diameter.

3. The third quality is distinctness or accuracy of representation. In order to produce this, the rays which pass through the extremities of the object-glass ought to meet in the same point with those which pass through the middle, or at least the aberration should not be perceptible. When a simple object-glass is employed, its focal distance must exceed a certain limit proportional to the magnifying power. Thus, if you wish to magnify 100 times, the focal distance of the object-glass must be at 'east 30 feet. It is the destination, therefore, which imposes the necessity of making telescopes so excessively long, if we want to obtain a very great magnifying power. Now, in order to remedy this defect, an object-glass composed of two lenses may be employed; and could artists succeed in the construction of them, we should be enabled very considerably to shorten telescopes, while the same magnifying power remained. You will have the goodness to recollect what I have already suggested at some length on this subject.

4. The fourth quality regards likewise the distinctness or purity of representation, as far as it is affected by the different refrangibility of rays of different colours. I have shown how that defect may be remedied; and as it is impossible that the images formed by different rays should be collected in a single one, the point in question is to arrange the lenses in the manner I have described in the preceding Letter; that is, the terminating line of the last images must pass through the eye. Without this, the telescope will have the defect of representing objects surrounded with the colours of the rainbow; but the defect will disappear on arranging the lenses in the method I have pointed out. But to this effect, more than two lenses must be employed, in order to a proper arrangement. I have

hitherto spoken only of telescopes with two lenses, one of which is the object-glass, and the other the eye-glass; and you know that their distance from each other is already determined by their focal distances, so that here we are not at liberty to make any alteration. It happens, fortunately, however, that the terminating line which I have mentioned passes nearly through the place of the eye, so that the defect arising from the colours of the rainbow is almost imperceptible, provided the preceding defect is remedied, especially when the magnifying power is not very great. But when the power is considerable, it would be proper to employ two eye-glasses, in order entirely to annihilate the colours of the rainbow, as in this case the slightest defects, being equally magnified, become insupportable.

5. The fifth and last good quality of a telescope is a large apparent field, or the space which the instrument discovers at once. You recollect that small pocket-glasses with a concave eye-glass are subject to the defect of presenting a very small field, which renders them incapable of magnifying greatly. The other species, that with a convex eye-glass, is less subject to this defect; but as it represents the object inverted, telescopes of the first species would be preferable, did they discover a larger field, which depends on the diameter of the aperture of the eye-glass; and you know we cannot increase this aperture at pleasure, because it is determined by focal distance. But by employing two or three, or even more eye-glasses, we have found means to render the apparent field greater; and this is an additional reason for employing several lenses in order to procure a telescope in all respects excellent.

To these good qualities another may be still added, that the representation shall not be inverted by the instrument, as by astronomical telescopes. But this defect may be easily remedied, if it be one, by the

addition of two more eye-glasses, as I shall show in my next Letter.

3*d* *April*, 1762.

LETTER CVI.

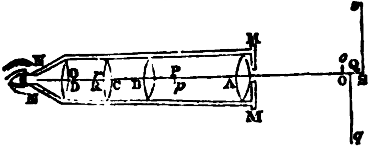
Terrestrial Telescopes with four Lenses.

I HAVE treated at considerable length of telescopes composed of two convex lenses, known by the name of astronomical tubes, because they are commonly used for observing the heavenly bodies.

You will readily comprehend that the use of such instruments, however excellent they may be, is limited to the heavens, because they represent objects in an inverted position, which is very awkward in contemplating terrestrial bodies, as we would rather wish to view them in their natural situation; but on the discovery of this species of telescope, means were quickly found of remedying that defect, by doubling, if I may say so, the same telescope. For as two lenses invert the object, or represent the image inverted, by joining a similar telescope to the former, for viewing the same image, it is again inverted, and this second representation will exhibit the object upright. Hence arose a new species of telescopes, composed of four lenses, called terrestrial telescopes, from their being designed to contemplate terrestrial objects; and the method of constructing them follows.

1. The four lenses A, B, C, D, *Fig.* 189, enclosed

Fig. 189.



in the tube M M N N, represent the telescope in question; the first of which, A, directed towards the object, is denominated the object-glass, and the other three, B C D, the eye-glass. These four lenses are all convex, and the eye must be placed at the extremity of the tube, at a certain distance from the last eye-glass D, the determination of which shall be afterward explained.

2. Let us consider the effect which each lens must produce when the object O *o*, which is viewed through the telescope, is at a very great distance. The object-glass will first represent the image of this object at P *p*, its focal distance, the magnitude of the image being determined by the straight line drawn from the extremity *o* through the centre of the lens A. This line is not represented in the figure, that it may not be embarrassed with too many lines.

3. This image P *p* occupies the place of the object with respect to the second lens B, which is placed in such a manner that the interval B P shall be equal to its focal distance, in order that the second image may be thence transported to an infinite distance, as Q *q*, which will be inverted as the first P *p*, and terminated by the straight line drawn from the centre of the lens B through the extremity *p*.

4. The interval between these two first lenses A, B is equal, therefore, to the sum of their focal distances; and were the eye placed behind the lens B, we should have an astronomical telescope, through which the object O *o* would be seen at Q *q*, and consequently inverted, and magnified as many times as the distance A P exceeds the distance B P. But instead of the eye, we place behind the lens B, at some distance, the third lens C, with respect to which the image Q *q* occupies the place of the object, as in fact it receives the rays from this image Q *q*, which being at a very great distance, the lens

C will represent the image of it, at its focal distance, in R *r*.

5. The image Q *q* being inverted, the image R *r* will be upright, and terminated by the straight line drawn from the extremity *q* through the centre of the lens C, which will pass through the point *r*. Consequently the three lenses A, B, C together represent the object O *o* at R *r*, and this image R *r* is upright.

6. Finally, we have only to place the last lens in such a manner that the interval D R shall be equal to its focal distance; this lens D will again transport the image R *r* to an infinite distance, as S *s*, the extremity of which *s* will be determined by the straight line drawn from the centre of the lens D through the extremity *r*; and the eye placed behind this lens will in fact see this image S *s* instead of the real object O *o*.

7. Hence it is easy to ascertain how many times this telescope, composed of four lenses, must magnify the object; you have only to attend to the two couple of lenses, A, B and C, D, each of which separately would be an astronomical telescope. The first pair of lenses A and B magnifies as many times as the focal distance of the first lens A exceeds that of the second lens B; and so many times will the image formed by it, Q *q*, exceed the real object O *o*.

8. Further, this image Q *q* occupying the place of the object with respect to the other pair of lenses C and D, it will be again multiplied as many times as the focal distance of the lens C exceeds that of the lens D. These two magnifying powers added give the whole magnifying power produced by the four lenses.

9. If, then, the first pair of lenses, A and B, magnify ten times, and the other pair, C and D, three times, the telescope will magnify the object thrice ten, that is, thirty times; and the aperture of the

object-glass A must correspond to this magnifying power, according to the rule formerly laid down.

10. Hence you see, then, that on separating from a terrestrial telescope the two last lenses C and D, there would remain an astronomical telescope, and that these two lenses C and D would likewise form such a telescope. A terrestrial telescope, therefore, consists of two astronomical ones; and reciprocally, two astronomical telescopes combined form a terrestrial one.

This construction is susceptible of endless variations, some preferable to others, as I shall afterward demonstrate.

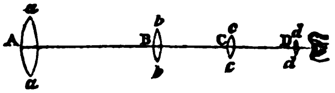
6th April, 1762.

LETTER CVII.

Arrangement of Lenses in Terrestrial Telescopes.

You have seen how, by the addition of two convex lenses to an astronomical telescope, a terrestrial one is produced, which represents the object upright. The four lenses of which a terrestrial telescope is composed are susceptible of an infinite variety of arrangement, with respect to both focus and distance. I shall explain those which are of most essential importance, and refer you to *Fig.* 196.

Fig. 196.



1. With respect to their distances, I have already remarked that the interval between the two first lenses, A and B, is the sum of their focal distances; and the same thing holds as to the last lenses C and

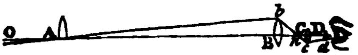
D: for each pair may be considered as a simple telescope, composed of two convex lenses. But what must be the interval between the two middle lenses B and C? May it be fixed at pleasure? As it is certain that whether this interval be great or small, the magnifying power, always compounded of the two which each pair would produce separately, must continue the same.

2. On consulting experience we soon perceive that when the two middle lenses are placed very near each other, the apparent field almost entirely vanishes; and the same thing takes place when they are too far separated. In both cases, to whatever object the telescope is pointed, we discover only a very small part of it.

3. For this reason artists bring the last pair of lenses nearer to the first, or remove them to a greater distance, till they discover the largest field, and delay fixing the lenses till they have found this situation. Now they have observed, that in settling this most advantageous arrangement, the distance of the middle lenses, B and C, is always greater than the sum of the focal distances of these same two lenses.

4. You will readily conclude that this distance cannot depend on chance, but must be supported by a theory, and that affording a termination much more exact than what experience alone could have furnished. As it is the duty of a natural philosopher to investigate the causes of all the phenomena which experience discovers, I proceed to unfold the true principles which determine the most advantageous distance B C between the two middle lenses. For this purpose I refer to *Fig.* 197.

Fig. 197.



5. As all the rays must be conveyed to the eye, let us attend to the direction of that one which, proceeding from the extremity O of the visible object, passes through the centre A of the object-glass; for unless this ray is conveyed to the eye, this extremity O will not be visible. Now this ray undergoes no refraction in the object-glass, for it passes through the centre A; it will therefore proceed in a straight line to the second lens, which it will meet in its extremity *b*, as this is the last ray transmitted through the lenses.

6. This ray, being refracted by the second lens, will change its direction so as to meet somewhere at *n* the axis of the lenses; this would have happened to be the focus of this lens, had the ray A *b* been parallel to the axis; but as it proceeds from the point A, its reunion with the axis at *n* will be more distant from the lens B than its focal distance.

7. We must now place the third lens C in such a manner that the ray, after having crossed the axis at *n*, may meet it exactly in its extremity *c*; from which it is evident, that the greater the aperture of this lens C is, the farther it must be removed from the lens B, and the greater the interval B C becomes: but, on the other hand, care must be taken not to remove the lens C beyond that point, as in this case the ray would escape it, and be transmitted no farther. This circumstance, then, determines the just distance between the two middle lenses B and C, conformably to experience.

8. This lens C will produce a new refraction of the ray in question, which will convey it precisely to the extremity *d* of the last eye-glass D, which, being smaller than C, will render the line *c d* somewhat convergent towards the axis, and will thus undergo, in the last lens, such a degree of refraction as will reunite it with the axis at less than its focal distance; and there it is exactly that the eye must be placed, in order to receive all the rays trans-

mitted through the lenses, and to discover the greatest field.

9. Thus we are enabled to procure a field whose diameter is almost twice as large as with an astronomical telescope of the same magnifying power. By means, then, of these telescopes with four lenses we obtain a double advantage; the object is represented upright, and a much larger field is discovered —both circumstances of much importance.

10. Finally, it is possible to find such an arrangement of these four lenses as, without affecting either of the advantages now mentioned, shall entirely do away the defect arising from the colours of the rainbow, and at the same time represent the object with all possible distinctness. But few artists can attain this degree of perfection.

10*th April*, 1762.

LETTER CVIII.

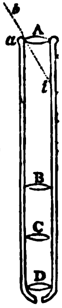
Precautions to be observed in the Construction of Telescopes. Necessity of blackening the Inside of Tubes. Diaphragms.

After these researches respecting the construction of telescopes, I must suggest and explain certain precautions necessary to be used; which, though they relate neither to the lenses themselves nor to their arrangement, are nevertheless of such importance, that if they are not very carefully observed, the best instrument is rendered entirely useless. It is not sufficient that the lenses should be arranged in such a manner that all the rays which fall upon them shall be transmitted through these lenses to the eye; care must be taken, besides, to prevent the transmission of extraneous rays through the telescope to disturb the representation. Let the following precautions, then, be taken.

1. The lenses of which a telescope is composed must be enclosed in a tube, that no other rays except those which are transmitted through the object-glass may reach the other lenses. For this effect, the tube must be so very close throughout that no chink admits the smallest portion of light. If by any accident the tube shall be perforated ever so slightly, the extraneous light admitted would confound the representation of the object.

2. It is likewise of importance to blacken throughout the inside of the telescope, of the deepest black possible, as it is well known that this colour does not reflect the rays of light, be they ever so powerful. You must have observed, accordingly, that the tubes of telescopes are always blackened internally. A single reflection will show the necessity of it.

Fig. 199.



3. The object-glass A, *Fig.* 199, transmits, not only the rays of the object represented by the telescope, but those also which by the extremities enter all around in great abundance; such is the ray *b a*, which falls on the inside upon the frame of the tube at *i*: if, therefore, the tube were white inwardly, or of any other colour, it would be illuminated by this ray, and of itself would generate new rays of light, which must of necessity be conveyed through the other lenses, and disturb the representation, by mingling with the proper rays of the object.

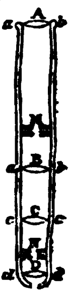
4. But if the inside of the tube be blackened deeply, no new rays will be produced, let the light be ever so strong. This blackening must be carried through the whole length of the telescope, as there is no black so deep as not to generate, when illuminated, some faint light. Supposing, then, that some extraneous rays were to make their way to the second lens B, the black of the tube, pursuing their course, would easily

absorb them altogether. There is a brilliant black, which, for this reason, it would be very improper to employ.

5. But even this precaution is not sufficient, it is necessary likewise to furnish the inside of the tube with one or more diaphragms, perforated with a small circular aperture, the better to exclude all extraneous light; but care must be taken that they do not exclude the rays of the object which the instrument is intended to represent. See *Fig.* 198.

6. It is necessary to observe at what place in the tube the proper rays of the object are most contracted; this must be at the points where their images are represented, for there all the rays are collected together. Now, the object-glass A represents the image in its focus at M. You have only, then, to compute the magnitude of this image, and there to fix your diaphragm, whose aperture *m n* shall be equal to the magnitude of the image, or rather somewhat greater. For if the aperture were less than the image, there would be a proportional loss of the apparent field, which is always a great defect.

Fig. 198.



7. These are the observations respecting the diaphragm which apply to astronomical telescopes composed of two convex lenses. In terrestrial telescopes two images are represented within the tube; besides the first at M, represented by the object-glass in its focus, and which the second lens B transports to an infinite distance, the third lens represents a second image in its focus N, which is upright, whereas the former was inverted. At N, therefore, is the proper place to fix a second diaphragm perforated with an aperture *n n*, of the magnitude of the image there represented.

8. These diaphragms, aided by the blackness of

the inside of the tube, produce likewise an excellent effect with respect to distinctness of representation. It must be carefully observed, however, that the greater the field is which the telescope discovers, the less is to be expected from these diaphragms, as in that case the images become greater, so that the aperture of the diaphragms must be so enlarged as to render them incapable of any longer excluding the extraneous rays. So much the greater care, therefore, must be taken thoroughly to blacken the inside of the tube, and to make it larger, which considerably diminishes the unpleasant effect of which I have been speaking.

13th April, 1762.

LETTER CIX.

In what manner Telescopes represent the Moon, the Planets, the Sun, and the Fixed Stars. Why these last appear smaller through the Telescope than to the naked Eye. Calculation of the Distance of the Fixed Stars, from a Comparison of their apparent Magnitude with that of the Sun.

I AM persuaded, that by this time you are very well pleased to be relieved at length from the dry theory of telescopes, which is rendered agreeable only by the importance of the discoveries which they have enabled us to make.

What pleasing surprise is felt on seeing very distant objects as distinctly as if they were one hundred times nearer to us, or more especially in cases where there is no possibility of reaching them, which holds with respect to the heavenly bodies! And you are already disposed to admit, that with the aid of the telescope many wonderful things relating to the stars have been discovered.

On viewing the moon one hundred times nearer

than she really is, many curious inequalities are discernible; such as excessive heights and profound depths, which from their regularity resemble rather works of art than natural mountains. Hence a very plausible argument is deduced to prove that the moon is inhabited by reasonable creatures. But we have proofs still more satisfactory in simply contemplating the almighty power, in union with the sovereign wisdom and goodness of the Great Creator.

Thus the most important discoveries have been made respecting the planets, which, to the unassisted eye, appear only as so many luminous points; but which, viewed through a good telescope, resemble the moon, and appear even still much greater.

But you will be not a little surprised, when I assure you that with the assistance of the best telescope, even one which magnifies more than two hundred times, the fixed stars still appear only as points, nay, still smaller than to the naked eye. This is so much the more astonishing, that it is certain the telescope represents them such as they would appear were we two hundred times nearer. Are we not hence reduced to the necessity of concluding, that here telescopes fail to produce their effect? But this idea presently vanishes, on considering that they discover to us millions of little stars which, without their aid, must have for ever escaped the eye. We likewise perceive the distances between the stars incomparably greater; for two stars which to the naked eye seemed almost to touch each other, when viewed through the telescope are seen at a very considerable distance; a sufficient proof of the effect of the telescope.

What, then, is the reason that the fixed stars appear to us smaller through the telescope than to the naked eye? In resolving this question, I remark, first, that the fixed stars appear greater to the naked eye than they ought to do, and that this arises from a false light occasioned by their twinkling. In fact, when the rays proceeding from a star come to paint their image at the bottom of the eye, on the retina, our nerves are struck by it only in one point; but by the lustre of the light the adjacent nerves likewise undergo a concussion, and produce the same feeling which would be communicated if the image of the object painted on the retina were much greater. This happens on looking, in the night, at a very distant light. It appears much greater than when we view it at a small distance; and this increase of magnitude is occasioned only by a false glare. Now, the more that a telescope magnifies, the more this accident must diminish; not only because the rays are thereby rendered somewhat fainter, but because the real image at the bottom of the eye becomes greater; so that it is no longer a single point which supports the whole impression of the rays. Accordingly, however small the stars may appear through a telescope, we may confidently affirm, that to the naked eye they would appear still much smaller but for this accidental false light, and that as many times as the telescope magnifies.

Hence it follows, that as the fixed stars appear only like so many points, though magnified more than 200 times, their distance must be inconceivable. It will be easy for you to form a judgment how this distance may be computed. The diameter of the sun appears under an angle of 32 minutes: if, therefore, the sun were 32 times farther off, he would appear under an angle of one minute; and, consequently, still much greater than a fixed star viewed through the telescope, the diameter of which does not exceed two seconds, or the thirtieth part of a minute. The sun, therefore, must be thirty times more, that is 960 times, farther removed, before his appearance could be reduced to that of a fixed star observed with the assistance of a telescope. But the fixed star is 200 times farther off than the tele-

scope represents it; and, consequently, the sun must be 200 times 960, that is, 192,000 times farther off than he is, before he could be reduced to the appearance of a fixed star. It follows, that if the fixed stars were bodies as large as the sun, their distances would be 192,000 times greater than that of the sun. Were they still greater, their distances must be still so many times greater; and supposing them even many times smaller, their distances must always be more than a thousand times greater than that of the sun. Now the distance of the sun from our globe is about 96,000,000 of English miles.

It is impossible, undoubtedly, to think of this immense distance of the fixed stars, and of the extent of the whole universe, without astonishment. What must be the power of that Great Being who created this vast fabric, and who is the absolute Master of it? Let us adore Him with the most profound veneration.

17th April, 1762.

LETTER CX.

Why do the Moon and the Sun appear greater at rising and setting than at a certain Elevation? Difficulties attending the Solution of this Phenomenon.

You must have frequently remarked, that the moon at rising and setting appears much larger than when she is considerably above the horizon; and every one must give testimony to the truth of this phenomenon. The same observation has been made with respect to the sun. This appearance has long been a stumbling-block to philosophers; and, viewed in whatever light, difficulties almost insuperable present themselves.

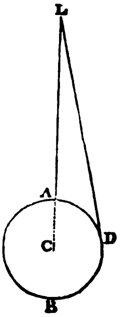
It would be ridiculous to conclude that the moon's body is really greater when she is in the horizon than when she has attained her greatest elevation. For, besides that such an idea would be absurd in itself, it must be considered, that when the moon appears to us in the horizon she appears to other inhabitants of our globe more elevated, and consequently smaller. Now, it is impossible that the same body should be at the same time greater and smaller.

It would be almost equally ridiculous to attempt the solution of this strange phenomenon by supposing that the moon is nearer to us when she appears in the horizon than when she is arrived at a great elevation, from our certain knowledge that a body appears greater in proportion as it is nearer us; and you know that the more distant any object is, the smaller it appears. It is for this reason precisely that the stars appear so extremely small, though their real magnitude be prodigious.

But however plausible this idea may seem, it is totally destitute of foundation; for it is undoubtedly certain, that the moon is at a greater distance from us at rising and setting, than when at a greater elevation. The demonstration follows: *Fig.* 200.

Let the circle A B D be the earth, and the moon at L. This being laid down, an inhabitant at A will see the moon in his zenith, or the most elevated point of the heavens. But another inhabitant at D, where the line D L touches the surface of the earth, will see the moon at the same time in his horizon; so that the moon will appear, at the same instant, to the spectator A in his zenith, and to the other spectator D in his horizon. It is evident, however, that the last distance D L is greater than the first A L, and consequently the moon is more distant from those who see her in the horizon than from those who see her near their

Fig. 200.



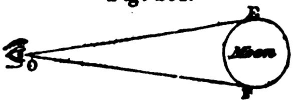
zenith. Hence it clearly follows, that the moon, when seen in the horizon, ought to appear smaller, being then in fact farther from us than when arrived at a great elevation. It is astonishing, therefore, that observation should be in direct contradiction to this, and that the moon should appear much greater when viewed near the horizon than in the summit of the heavens.

The more this phenomenon is investigated, the more strange it appears, and the more worthy of attention: it being undoubtedly certain that the moon, when most remote, that is, in the horizon, ought to appear smaller, whereas, nevertheless, every one is decidedly of opinion that she then appears considerably greater. This contradiction is evident, and even seems to overturn all the principles laid down in optics, which, however, are as clearly demonstrable as any in geometry.

I have purposely endeavoured to set this difficulty in its strongest light, in order to make you the more sensible of the importance of the true solution. Without entering into a discussion of this universal judgment, formed from appearances, respecting the prodigious magnitude of the moon in the horizon, I shall confine myself to the principal question: Is it true, in fact, that the moon, when near the horizon, actually appears greater?

You know that we are possessed of infallible means of exactly measuring the heavenly bodies, by ascertaining the number of degrees and minutes which they occupy in the heavens; or, which amounts to the same thing, by measuring, *Fig.* 201, the angle E O F, formed by the lines E O and F O,

Fig. 201.



drawn from the opposite points of the moon to the eye of the spectator O; and this angle E O F is what we call the apparent diameter of the moon. We have likewise instruments perfectly adapted to the purpose of exactly determining this angle. Now, when we employ such an instrument in measuring the moon's diameter, first at her rising, and afterward, when she has gained her greatest elevation, we actually find her diameter somewhat less in the first case than in the other, as the inequality of distance requires. There cannot remain the shadow of doubt as to this; but, for that very reason, the difficulty, instead of diminishing, gathers strength; and it will be asked with so much the more eagerness, How comes it that the whole world agrees in imagining the moon to be greater when rising or setting, though her apparent diameter is then in reality smaller? and, What can be the reason of this delusion, to which men are universally subject? The astronomer, who knows perfectly well that the moon's apparent diameter is then smaller, falls nevertheless into the same deception as the most ignorant clown.

20th April, 1762.

LETTER CXI.

Reflections on the Question respecting the Moon's apparent Magnitude. Progress towards a Solution of the Difficulty. Absurd Explanations.

You would scarcely have believed that the simple appearance of the moon involved so many difficulties; but I hope I shall be able to clear the way towards a solution, by the following reflections:—

1. It is not astonishing that our judgment respecting the magnitude of objects should not always be in correspondence with the visual angle under which

we see it: of this daily experience furnishes sufficient proof. A cat, for example, appears, when very near, under a greater angle than an ox at the distance of 100 paces. I could never, at the same time, imagine the cat to be larger than the ox: and you will please to recollect, that our judgment respecting magnitude is always intimately connected with that of distance; so that if we commit a mistake in the calculation of distance, our judgment respecting magnitude becomes, of necessity, erroneous.

2. In order to elucidate this more clearly, it sometimes happens that a fly passing suddenly before the eye, without our thinking of it, if our sight is fixed on a distant object we imagine at first that the fly is at a great distance; and as it appears under a very considerable angle, we take it for a moment to be a large fowl, which at the proper distance would appear under the same angle. It is then incontestably certain, that our judgment respecting the magnitude of objects is not regulated by the visual angle under which they are seen, and that there is a very great difference between the apparent magnitude of objects and the calculated or computed magnitude. The first is regulated by the visual angle, and the other depends on the distance to which we suppose the object to be removed.

3. To avail myself of this remark, I further observe, that we ought not to say that we see the moon greater in the horizon than at a considerable elevation. This is absolutely false, for we then see her even somewhat less. But, to speak accurately, we ought to say that we judge and compute the moon greater when she is in the horizon; and this is literally true with the unanimous consent of all mankind. This is sufficient to reconcile the apparent contradiction formerly suggested; for nothing prevents our judging or computing the moon to be greater when she rises or sets, though she is seen under a smaller visual angle.

4. We are no longer, then, called upon to explain why we see the moon greater in the horizon, which is impossible, for in reality she then appears smaller, as may be demonstrated by measuring the visual angle. The difficulty, therefore, is reduced to this: Wherefore do we judge or compute the moon to be greater when in those situations? or rather, we must endeavour to account for this whimsical computation. The thing is not surprising in itself, as we know a thousand cases in which we estimate objects to be very great, though we see them under very small angles.

5. We have only to say, then, that when the moon is rising or setting, we suppose her to be at a greater distance than when she has attained a certain elevation. Whenever this computation is settled, whatever may be the cause of it, the consequence is necessary, that we must likewise conclude the moon to be greater in proportion. For, in every case, the more distant we estimate any object to be, the greater we presume it is, and this in the same proportion. As soon as I imagine, by whatever illusion, that a fly passing close before my eye is at the distance of 100 paces, I am obliged, almost whether I will or not, to suppose it as many times greater as 100 paces exceed the real distance of the fly from my eyes.

6. We are now, therefore, reduced to a new question: Wherefore do we presume that the moon is at a greater distance when she is seen in the horizon? and, Wherefore is this illusion so universal as not to admit of a single exception? For the illusion of imagining that the moon is then at a much greater distance is altogether unaccountable. It is undoubtedly true that the moon is then really a little more distant, as I demonstrated in my last Letter; but the difference is so trifling as to be imperceptible. Besides, the sun, though 100 times more distant than the moon, does not appear so, and the eye

estimates even the fixed stars as nearly at the same distance.

7. Though, therefore, when the moon is in the horizon, she is actually a little more distant, this circumstance cannot affect the present question; and this universal computation, which induces the whole world to imagine the moon to be then at a much greater distance than she really is, must be founded on reasons entirely different, and capable of producing universal illusion. For, as the computation is unquestionably erroneous, the reasons which determine us to make it must necessarily be very striking.

8. Some philosophers have attempted to explain this phenomenon by alleging that it is occasioned by the intervention of various objects between us and the moon, such as cities, villages, forests, and mountains. This, say they, is the reason that she then appears to be much farther off; whereas, when she has attained a considerable elevation, as no other body intervenes, she must appear to be nearer. But this explanation, however ingenious it may at first sight appear, is destitute of solidity. On looking at the moon in the horizon through a small aperture made in any body which shall conceal the intermediate objects, she nevertheless still seems greater. Besides, we do not always imagine that objects between which and us many other bodies interpose are more distant. A great hall, for example, when quite empty, usually appears much larger than when filled with company, notwithstanding the numerous objects then interposed between us and the walls of the apartment.

24th April, 1762.

LETTER CXII.

An Attempt towards the true Explanation of this Phenomenon,—The Moon appears more distant when in the Horizon than when at a great Elevation.

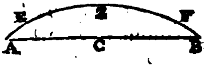
We are still, then, very far from the true solution of this universal illusion, under which all, without exception, are induced to imagine the moon to be much greater when in the horizon than when considerably elevated. I have already remarked, that this phenomenon is so much the more unaccountable, from its being demonstrable that the moon's apparent diameter is then even somewhat less: we ought not, therefore, to say, that we then see the moon greater, but that we imagine her to be so.

Accordingly, I have very often observed our judgment of objects to differ very widely from vision itself. We do not hesitate, for example, to conclude that a horse 100 paces distant is larger than a dog one pace distant, though the apparent magnitude of the dog is unquestionably greater; or, which amounts to the same thing, though the image of the dog painted on the bottom of the eye be greater than that of the horse. Our judgment in this case is regulated by taking distance into the account; and laying it down that the horse is much farther off than the dog, we conclude he is much larger.

It is very probable, therefore, that the same circumstance may take place respecting the moon's appearance, and induce us to reckon the moon greater when in the horizon than at a considerable elevation. In the case of the horse, our computation of distance was founded in truth; but here, as it is absolutely erroneous, the illusion must be singularly unaccountable, but must, at the same time, have a certain foundation, as its prevalence is universal, and cannot therefore be imputed to caprice. Wherein can it consist? This is to be the subject of our present inquiry.

1. Every one considers the azure expanse of heaven as a flattened arch, the summit of which is much nearer to us than the under part, where it meets the horizon. A person, accordingly, standing on a plane A B, *Fig.* 202, which extends as far as his sight, perceives the vault of heaven, commonly called the firmament, under the figure A E F B, in which the distances C A and C B are much greater than from the *zenith* to C.

Fig. 202.



2. This idea is likewise beyond all question a mere illusion, there being in reality no such vault surrounding and enclosing us on every side. It is a void of immense extent, as it reaches to the most distant of the fixed stars—an interval that far exceeds all power of imagination. I use the word *void*, to distinguish it from gross terrestrial bodies. For, near the earth, space is occupied by our atmosphere; and beyond, by that fluid, infinitely more subtile, which we call *ether*.

3. Though this vault, however, has no real existence, it possesses an undoubted reality in our imagination; and all mankind, the philosopher as well as the clown, are subject to the same illusion. On the surface of this arch we imagine the sun, the moon, and all the stars to be disposed like so many brilliant studs affixed to it; and though we have a perfect conviction of the contrary, we cannot help giving way to the illusion.

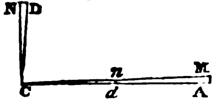
4. This being laid down, when the moon is in the horizon, imagination attaches her to the point A or B of this supposed vault, and hence we conclude her distance to be as much greater as we consider the line C A or C B to be greater than C Z; but when

she ascends and approaches the zenith, we imagine she comes nearer; and if she reaches the very zenith, we think she is at the least possible distance.

5. The illusion as to distance necessarily involves that which respects magnitude. As the moon at A appears much farther from C than in the zenith, we are in a manner forced to conclude that the moon is really so much greater; and that in the same proportion that the distance C A appears to exceed the distance C Z. All will not, perhaps, agree in determining this proportion; one will say, the moon appears to him twice as great when in the horizon; another will say three times; and the generality will declare for the medium between two and three; but every one will infallibly agree in asserting that the moon appears larger.

6. It may be necessary here to present you with the demonstration of this proposition. The computation of magnitude is necessarily involved in the computation of distance. When the moon is near the horizon, we see her, *Fig.* 203, under a certain angle, say M C A, the spectator being at C; and when she is at a very great elevation, let N C D be the angle under which we see her. It is evident that these two angles M C A and N C D are nearly equal to each other, the difference being imperceptible.

Fig. 203.



7. But, in the first case, as we estimate the moon's distance to be much greater, or equal to the line C A, with reference to the imaginary vault above described, it follows, that we compute the moon's diameter to be equal to the line M A. But, in the other case, the distance of the moon C D appears much smaller; and consequently, as the angle N C D is equal to the angle M C A, the computed magni-

tude D N will be much smaller than the computed magnitude A M.

8. To put this beyond a doubt, you have only to cut off from the lines C M and C A the parts C *d* and C *n*, equal to the lines C D and C N; and as in the two triangles C *d n* and C D N, the angles at the point C are equal, the triangles themselves are likewise so, and consequently the line D N will be equal to the line *d n*; but *d n* is evidently smaller than A M, and that as many times as the distance C *d* and C D is less than C A. This is a clear demonstration of the reason why we estimate the moon to be greater when in the horizon than when near the zenith.

29th April, 1762.

LETTER CXIII.

The Heavens appear under the form of an Arch flattened towards the Zenith.

You will charge me, no doubt, with pretending to explain one illusion by another equally unaccountable. It may be said, that the imaginary vault of heaven is altogether as inconceivable as the increased appearance of the moon and the other heavenly bodies when in or near the horizon. The objection is not without foundation, and therefore lays me under the necessity of attempting to explain the true reason why the heavens appear in the form of an arch flattened towards the summit. The following reflections may, perhaps, be received as an acquittance of my engagement.

1. In order to account for this imaginary vault, it will be alleged that it proceeds from the appearance of the heavenly bodies, as seeming more remote when in the horizon than when near to or in the zenith. This is undoubtedly a formal *petitio prin-*

cipii, as logicians call it, or a begging of the question, which every one is entitled to reject as a ground of reasoning. In truth, having said above that the imaginary vault of heaven makes the moon in the horizon appear farther off than when near the zenith, it would be ridiculous to affirm, that the thing which leads us to imagine the existence of such a vault is that horizontal objects appear more distant than vertical.

2. It was not, however, useless to suggest the idea of this imaginary vault, though it may not carry us a great way forward; and after I shall have explained wherefore the heavenly bodies appear more remote when viewed near the horizon, you will be enabled to comprehend, at the same time, the reason of that twofold universal illusion, namely, the apparently increased magnitude of the heavenly bodies when in the horizon, and the flattened arch of heaven.

3. The whole, then, reverts to this, to explain wherefore the heavenly bodies when seen in the horizon appear more remote than when at a considerable elevation. I now affirm, it is because these objects appear less brilliant; and this imposes on me the double task of demonstrating why these objects display less brilliancy when in or near the horizon, and of explaining how this circumstance necessarily involves the idea of a greater distance. I flatter myself I shall be enabled to discharge both of these to your satisfaction.

4. The phenomenon itself will not be called in question. However greater the sun's lustre may be at noon, which it is then impossible to ascertain, you know that in the morning and evening, when he is rising or setting, it is possible to contemplate his body without any injury to the eye; and the same thing takes place with respect to the moon and all the stars, whose brilliancy is greatly diminished in the vicinity of the horizon. We accord-

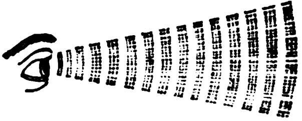
ingly do not see the smaller stars when at a small elevation above the horizon, though they are sufficiently discernible at a certain height.

5. This being established beyond a possibility of doubt, the cause of this difference of illumination remains to be investigated. It is abundantly evident that we can trace it only in our atmosphere, or the body of air which encompasses our earth, in so far as it is not perfectly transparent. For if it were, so that all the rays should be transmitted through it without undergoing any diminution, there could be no room to doubt that the stars must always shine with the same lustre, in whatever region of the heavens they might be discovered.

6. But the air, a substance much less fine and subtile than ether, whose transparency is perfect, is continually loaded with heterogeneous particles, rising into it above the earth, such as vapours and exhalations, which destroy its transparency; so that if a ray should fall in with such a particle, it would be intercepted, and almost extinguished by it. It is accordingly evident, that the more the air is loaded with such particles, which prevent the transmission of light, the more rays must be lost by the interception; and you know that a very thick mist deprives the air of almost all its transparency, to such a degree that it is frequently impossible to distinguish objects at three paces' distance.

7. Let the points marked in *Fig.* 204 represent such particles scattered through the air, whose number is greater or less, according as the air is more or less transparent. It is evident, that many

Fig. 204.



of the rays which pervade that space must be lost, and that the loss must be greater in proportion as the space which they had to run through that air is greater. We see, then, that distant objects become invisible in a fog, while such as are very near the eye may be still perceptible, because the rays of the first meet in their progress a greater number of particles which obstruct their transmission.

8. We must hence conclude, that the longer the space is through which the rays of the heavenly bodies have to pass through the atmosphere in order to reach our eyes, the more considerable must be their loss or diminution. Of this you can no longer entertain any doubt. All that remains, then, is simply to demonstrate, that the rays of the stars which we see in or near our horizon have a longer space of the atmosphere to pervade than when nearer the zenith. When this is done, you will easily comprehend why the heavenly bodies appear much less brilliant when near the horizon than at the time of rising and setting. This shall be the subject of my next Letter.

1st May, 1762.

LETTER CXIV.

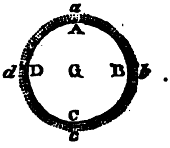
Reason assigned for the Faintness of the Light of the Heavenly Bodies in the Horizon.

What I have just advanced, namely, that the rays of the heavenly bodies, when in the horizon, have a larger portion of our atmosphere to pervade, may appear somewhat paradoxical, considering that the atmosphere universally extends to the same height, so that at whatever point the star may be, its rays must always penetrate through the whole of that height before it can reach our eyes. The following

reflections, I flatter myself, will give you complete satisfaction on the subject.

1. It is first of all necessary to form a just idea of the atmosphere which surrounds our globe. For this purpose the interior circle A B C D, *Fig.* 205, shall represent the earth, and the exterior dotted circle *a b c d* shall mark the height of the atmosphere. Let it be remarked, that universally in proportion as the air rises above the surface of the earth it becomes always more transparent and subtile, so that at last it is imperceptibly lost in the ether which fills the whole expanse of heaven.

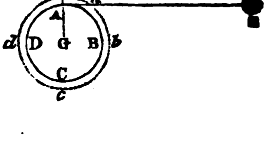
Fig. 205.



2. The grosser air, that which is most loaded with the particles that intercept and extinguish the rays of light, is universally found in the lower regions, near the surface of the earth. It becomes, therefore, more subtile as we ascend, and less obstructive of the light; and at the height of 5 English miles has become so transparent as to occasion no perceptible obstruction whatever of the light. The distance, then, between the interior circle and the exterior, may be fixed at 5 English miles nearly, whereas the semi-diameter of the globe contains about 3982 of such miles; so that the height of the atmosphere is a very small matter compared with the magnitude of the globe.

3. Let us now consider, *Fig.* 206, a spectator at A, on the surface of the earth; and drawing from the centre of the globe G, through A, the line G Z, it will be directed towards the zenith of the spectator. The line A S, which is

Fig. 206.



perpendicular, and touches the earth, will be horizontal to it. Consequently, he will see a star at Z in his zenith, or in the summit of the heavens; but a star at S will appear to him in the horizon at its rising or setting. Each of these stars may be considered as infinitely distant from the earth, though it was impossible to represent this in the figure.

4. Now you have only to cast your eye once more on the figure, to be satisfied that the rays proceeding from S have a much longer space to travel through the atmosphere than those from the star Z, before they reach the spectator at A. Those from the star Z have only to pass through the perpendicular height of the atmosphere *a* A, which is not above 5 English miles, whereas those that come from the star S have to travel the whole space *h* A, which is evidently much longer; and could the figure be represented more conformably to the fact, so as to exhibit the radius G A 3982 times longer than the height A *a*, we should find the distance A *h* to exceed 40 such miles.

5. It is further of importance to remark, that the rays of the star Z have but a very small space to travel through the lower region of the atmosphere, which is most loaded with vapour; whereas the rays of the star S have a much longer course to perform through that region, and are obliged to graze, if I may use the expression, along the surface of the earth. The conclusion, then, is obvious. The rays of the star Z undergo scarcely any diminution of lustre, but those of the star S must be almost extinguished, from so long a passage through the grosser air.

6. It is indisputably certain, then, that the stars which we see in the horizon must appear with a lustre extremely diminished; and it will simply account to you for a well-known fact, that you can, without any inconvenience, fix your eyes steadily on the rising or setting sun; whereas, at noon, or at a considerable elevation, his lustre is insupport-

able. This is the first point I undertook to demonstrate; I proceed to the second, namely, to prove that it is the diminution of light which forces us almost to imagine the heavenly bodies at a much greater distance than when we see them in all their lustre.

7. The reason must be sought in terrestrial bodies, with which we are every day conversant, and respecting whose distance we form a judgment. But for the same reason that rays of light in passing through the air undergo some diminution of lustre, it is evident that the farther an object is removed from us, the more of its lustre it loses, and the more obscure it becomes in proportion. Thus, a very distant mountain appears quite dark; but on a nearer approach we can easily discover trees on it, and other minuter objects, which it was impossible to distinguish at a very remote distance.

8. This observation, so general, and which never misleads us in contemplating terrestrial bodies, has produced in us from our childhood this fundamental principle, from which we conclude objects to be distant in proportion as the rays of light which they emit are weakened. It is in virtue of this principle, therefore, that we conclude the moon to be farther off at rising and setting than at a considerable elevation; and for the same reason we conclude she is so much greater. You will, I flatter myself, admit this reasoning to be solid, and this embarrassing phenomenon to be as clearly elucidated as the nature of the subject permits.

4th May, 1762.

LETTER CXV.

Illusion respecting the Distance of Objects, and the Diminution of Lustre.

The principle of our imagination, by which I have endeavoured to explain the phenomenon of the moon's greater apparent magnitude in the horizon than at a considerable elevation, is so deeply rooted in our nature as to become the source of a thousand similar illusions, some of which I will take the liberty to suggest.

We have been habituated from infancy, almost involuntarily, to imagine objects to be distant in proportion as their lustre is diminished; and, on the other hand, very brilliant objects appear to be nearer than they really are. This illusion can proceed only from an ill-regulated imagination, which very frequently misleads us. It is nevertheless so natural and so universal that no one is capable of guarding against it, though the error, in many cases, is extremely palpable, as I have shown in the instance of the moon; but we are equally deceived in a variety of other instances, as I shall presently make appear.

1. It is a well-known illusion that the flame of a conflagration in the night appears much nearer than it really is. The reason is obvious; the fire blazes in all its lustre; and in conformity to a principle pre-established in the imagination, we always conclude it to be nearer than it is in reality.

2. For the same reason a great hall, the walls of which are perfectly white, always appears smaller. White, you know, is the most brilliant colour: hence we conclude the walls of such an apartment to be too near; and consequently the apparent magnitude is thereby diminished.

3. But in an apartment hung with black, as is the custom in mournings, we perceive the directly opposite effect. The apartment now appears considerably more spacious than it really is. Black is undeniably the most gloomy of colours, for it reflects scarcely any light on the eye; hence the walls of an apartment in deep mourning seem more distant than they are, and consequently greater; but let the black hangings be removed and the white colour reappear, and the apartment will seem contracted.

4. No class of men avail themselves more of this natural and universal illusion than painters. The same picture, you know, represents some objects as at a great distance, and others as very near; and here the skill of the artist is most conspicuous. It is not a little surprising, that though we know to absolute certainty all the representations of a picture to be expressed on the same surface, and consequently at nearly the same distance from the eye, we should be, nevertheless, under the power of illusion, and imagine some to be quite near, and others extremely distant. This illusion is commonly ascribed to a dexterous management of light and shade, which undoubtedly furnish the painter with endless resources. But you have only to look at a picture to be sensible that the objects intended to be thrown to a great distance are but faintly and even indistinctly expressed. Thus, when the eye is directed to very remote objects, we easily perceive, for example, that they are men; but it is impossible to distinguish the parts, such as the eyes, the nose, the mouth; and it is in conformity to this appearance that the painter represents objects. But those which he intends should appear close to us he displays in all the brightness of colouring, and is at pains clearly to express each minute particular. If they are persons, we can distinguish the smallest lineaments of the face, the folds of the drapery, &c.: this part

of the representation seems, I may say, to rise out of the canvass, while other parts appear to sink and retire.

5. On this illusion, therefore, the whole art of painting entirely rests. Were we accustomed to form our judgment in strict conformity to truth, this art would make no more impression on us than if we were blind. To no purpose would the painter call forth all his powers of genius, and employ the happiest arrangement of colours; we should coldly affirm, on that piece of canvass there is a red spot, here a blue one; there a black stroke, here some whitish lines; every thing is on the same plane surface; there is no rising nor sinking; therefore no real object can be represented in this manner: the whole would in this case be considered as a scrawling on paper, and we should perhaps fatigue ourselves to no purpose in attempting to decipher the meaning of all these different coloured spots. Would not a man in such a state of perfection be an object of much compassion, thus deprived of the pleasure resulting from the productions of an art at once so amusing and so instructive?

8th May, 1762.

LETTER CXVI.

On the Azure Colour of the Heavens.

You are now enabled to comprehend the reason why the sun and moon appear much greater when in the horizon than at a considerable elevation. It consists in this, that we then unintentionally compute these bodies to be at a greater distance, a computation founded on the very considerable diminution which their lustre in that position undergoes, from the longer passage which the rays have to force through the lower region of the atmosphere, which is the most loaded with vapours and exhalations, whereby the transparency is diminished. This is a brief recapitulation of the reflections which I have taken the liberty to suggest on this subject.

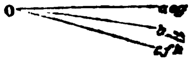
This quality of the air, which diminishes transparency, might at first sight be considered as a defect. But on attending to consequences, we shall find it so far from being such, that we ought, on the contrary, to acknowledge in it the infinite wisdom and goodness of the CREATOR. To this impurity of the air we are indebted for that wonderful and ravishing spectacle which the azure of the heavens presents to the eye; for the opaque particles which obstruct the rays of light are illuminated by them, and afterward retransmit their own proper rays, produced in their surface by a violent agitation, as is the case in all opaque bodies. Now, it is the number of vibrations communicated to them which represents to us this magnificent azure; a circumstance which well deserves to be completely unfolded.

1. I observe, first, that these particles are extremely minute and considerably distant from each other, besides their being delicately fine and almost wholly transparent. Hence it comes to pass, that each separately is absolutely imperceptible, so that we can be affected by them only when a very great number transmit their rays at once to the eye, and nearly in the same direction. The rays of several must therefore be collected, in order to excite a sensation.

2. Hence it clearly follows, that such of these particles as are near to us escape our senses, for they must be considered as points dispersed through the mass of air.

But such as are very distant from the eye, as, *Fig.* 207, the points *a b c*, collect in the eye O, almost according to the same direction, their several rays, which thus

Fig. 207.



become sufficiently strong to affect the sight, especially when it is considered that similar particles more remote, *e f g h*, as well as others more near, concur in producing this effect.

3. The azure colour which we see in the heavens when serene is nothing else, then, but the result of all these particles dispersed through the atmosphere, especially of such as are very remote: it may be affirmed, therefore, that they are in their nature blue, but a blue extremely clear, which does not become sufficiently deep and perceptible, except when they are in a very great number, and unite their rays according to the same direction.

4. Art has the power of producing a similar effect. If, on dissolving a small quantity of indigo in a great quantity of water, you let that water fall drop by drop, you will not perceive in the separate drops the slightest appearance of colour; and on pouring some of it into a small goblet, you will perceive only a faint bluish colour. But if you fill a large vessel with the same water, and view it at a distance, you will perceive a very deep blue. The same experiment may be made with other colours. Burgundy wine, in very small quantities, appears only to be faintly reddish; but in a large flask completely filled, the wine appears of a deep red.

5. Water, in a large and deep vessel, presents something like colour; but in a small quantity is altogether clear and limpid. This colour is commonly more or less of a greenish cast, which may warrant us in saying that the minute particles of water are likewise so, but of a colour so delicately fine that a great mass of it must be collected before the colour can be perceptible, because the rays of a multitude of particles then concur towards producing this effect.

6. As it appears probable, from this observation, that the minute particles of water are greenish, it might be maintained, that the reason why the sea,

or the water of a lake or a pool, appears green, is the very same that gives the heavens the appearance of azure. For it is more probable that all the particles of the air should have a faintly bluish cast, but so very faint as to be imperceptible till presented in a prodigious mass, such as the whole extent of the atmosphere, than that this colour is to be ascribed to vapours floating in the air, but which do not appertain to it.

7. In fact, the purer the air is, and the more purged from exhalation, the brighter is the lustre of heaven's azure; which is a sufficient proof that we must look for the reason of it in the nature of the proper particles of the air. Extraneous substances mingling with it, such as exhalations, become, on the contrary, injurious to that beautiful azure, and serve to diminish its lustre. When the air is overloaded with such vapours, they produce fogs near the surface, and entirely conceal from us the azure appearance; when they are more elevated, as is frequently the case, they form clouds, which frequently cover the whole face of the sky, and present a very different colour from that of this azure of the pure air. This, then, is a new quality of air, different from those formerly explained—subtilty, fluidity, and elasticity; namely, the minute particles of air are in their nature bluish.

11*th May*, 1762.

LETTER CXVII.

What the Appearance would be were the Air perfectly transparent.

Independent of the beautiful spectacle of the azure heavens procured for us by this colour of the circumambient air, we should be miserable in the extreme were it perfectly transparent, and divested of those bluish particles; and we have here a new reason for adoring the infinite wisdom and goodness of the CREATOR.

That you may have full conviction of the truth of my assertion, let us suppose the air to be quite transparent, and similar to the ether, which, we know, transmits all the rays of the stars, without intercepting so much as one, and contains no particles themselves illuminated by rays, for such a particle could not be so without intercepting some of the rays which fell upon it. If the air were in this state, the rays of the sun would pass freely through it, without the retransmission of any light to the eye: we should receive, then, those rays only which came to us immediately from the sun. The whole heavens, except the spot occupied by the sun, would appear, therefore, completely dark; and instead of this brilliant blue, we should discover nothing on looking upward but the deepest black and the most profound night.

Fig. 208.

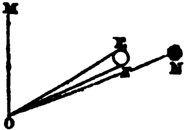


Fig. 208 represents the sun E F, and the point O is the eye of a spectator, which would receive from above no other rays but those of the sun, so that all illumination would be limited to the space of the small angle E O F. On directing the eye towards any other quarter of the heavens, say towards M, not a single ray would be emitted from it, and the appearance would be the same as if we looked into total darkness; now every place which transmits no ray of light is black. But here the stars must be excepted, which are spread over the whole face of the heavens; for on directing the eye towards M, nothing need prevent the rays of the stars which may be in that quarter from entering into it; nay, they would have even still more force, as they could suffer no diminution of lustre from the atmosphere, such as I am now supposing it. All the

stars, therefore, would be visible at noon-day, as in the darkest night; but it must be considered that this whole ray would be reduced to the space of the little angle E O F; all the rest of the heavens would be black as night.

At the same time, stars near the sun would be invisible; and we should not be able to see, for example, the star N, for on looking to it the eye would likewise receive the rays of the sun, with which it must be struck so forcibly that the feeble light of the star could not excite any sensation. I say nothing of the impossibility of keeping the eye open in attempting to look towards N. This is too obvious not to be understood.

But on opposing to the sun an opaque body, which shall intercept his rays, you could not fail to see the star N, however near it might be to the sun. It is easy to comprehend in what a dismal state we should then be. This proximity of lustre insupportable and darkness the most profound must destroy the organs of vision, and quickly reduce us to total blindness. Of this some judgment may be formed from the inconvenience we feel on passing suddenly from darkness into light.

Now this dreadful inconvenience is completely remedied by the nature of the air, from its containing particles opaque to a very small degree, and susceptible of illumination. Accordingly, the moment the sun is above the horizon, nay, somewhat earlier, the whole atmosphere becomes illuminated with his rays, and we are presented with that beautiful azure which I have described, so that our eyes, whichever way directed, receive a great quantity of rays generated in the same particles. Thus, on looking towards M, *Fig.* 208, p. 411, we perceive a great degree of light produced by this brilliant azure of the heavens.

This very illumination of the atmosphere prevents our seeing the stars by day: the reason of this is obvious. It far exceeds that of the stars, and the greater light always makes the lesser to disappear; and the nerves of the retina at the bottom of the eye, being already struck by a very strong light, are no longer sensible to the impression made by the feebler light of the stars.

You will please to recollect that the light of the full moon is upwards of 200,000 times more faint than that of the sun; and this will convince you that the light proceeding from the stars is a mere nothing in comparison with the light of the sun. But the illumination of the heavens in the day-time, even though the sun should be overclouded, is so great as many thousand times to exceed the light of the full moon.

You must have frequently perceived that in the night when the moon is full, the stars appear much less brilliant, and that those only of superior magnitude are visible, especially in the moon's vicinity; a sufficient proof that the stronger light always absorbs the feebler.

It is then an unspeakable benefit, that our atmosphere begins to be illuminated by the sun even before he rises, as we are thereby prepared to bear the vivacity of his rays, which would otherwise be insupportable, that is, if the transition from night to day were instantaneous. The season during which the atmosphere is gradually illuminated before sun-rising, and continues to be illuminated after he sets, is denominated twilight. This subject, from its importance, merits a particular explanation, which I propose to attempt in my next Letter; and thus one article in physics naturally runs into another.

15th May, 1762.

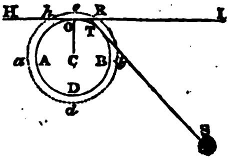
LETTER CXVIII.

Refraction of Rays of Light in the Atmosphere, and its Effects. Of the Twilight. Of the apparent rising and setting of the Heavenly Bodies.

IN order to explain the cause of the twilight, or that illumination of the heavens which precedes the rising of the sun, and continues some time after he is set, I must refer you to what has been already demonstrated respecting the horizon and the atmosphere.

Let the circle A O B D, *Fig.* 209, represent the earth, and the dotted circle *a o b d* the atmosphere; let a point O be assumed on the surface of the earth, through which draw the straight line H O R I, touching the earth at O, and this line H I will represent the horizon, which separates that part of the heavens which is visible to us from that which is not. As soon as the sun has reached this line, he appears in the horizon, both at rising and setting, and the whole atmosphere is then completely illuminated. But let us suppose the sun before his rising to be still under the horizontal line at S; from which the ray S T R, grazing the earth at T, may reach the point of the atmosphere situated in our horizon; the opaque particles which are there will already be illuminated by that ray, and consequently have become visible. Accordingly, some time before the rising of the sun, the atmosphere *h o* R over our horizon begins to be illuminated at R; and in proportion as the sun approaches the horizon a

Fig. 209.



greater part of it will be illuminated, till it becomes at length completely luminous.

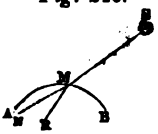
This reflection leads me forward to another phenomenon equally interesting, and very intimately connected with it, namely, that the atmosphere discovers to us the body of the sun and of the other stars some time before they get above the horizon, and some time after they have fallen below, by means of the refraction which rays of light undergo on passing from the pure ether into the grosser air which constitutes our atmosphere; of this I proceed to give you the demonstration.

1. Rays of light do not continue to proceed forward in a straight line any longer than they move through a transparent medium of the same nature. As soon as they pass from one medium to another, they are diverted from their rectilinear direction—their path is, as it were, broken off; and this is what we call refraction, which I formerly explained at considerable length, and demonstrated that rays, on passing from air into glass, and reciprocally, are thus broken or refracted.

2. Now air being a different medium from ether, when a ray of light passes from ether into air it must of necessity undergo some refraction.

Thus, the arch of the circle A M B, *Fig.* 210, terminating our upper atmosphere, if a ray of light M S, from the ether, falls upon it at M, it will not proceed straight forward in the same direction M N, but will assume, on entering into the air, the direction M R, somewhat different from M N; and the angle N M R is denominated the angle of refraction, or simply the refraction.

Fig. 210.

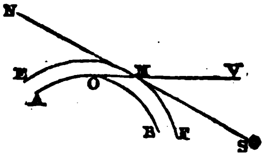


3. I have already remarked, that the refraction is greater in proportion as the ray S M falls more obliquely on the surface of the atmosphere, or as the

angle B M S is smaller or more acute. For if the ray S M falls perpendicularly on the surface of the atmosphere, that is, if the angle B M S is a right angle, no refraction will take place, but the ray will pursue its progress in the same straight line. This rule is universally applicable to every kind of refraction, whatever may be the nature of the two media through which the rays travel.

4. Let the arch of the circle A O B, *Fig.* 211, represent the surface of the earth, and the arch E M F terminate the atmosphere. If you draw at O the line O M V, touching the surface of the earth at O, it will be horizontal. And if the sun is still under the horizon at S, so as to be still invisible (for no one of his rays can yet reach us in a straight line), the ray S M being continued in a straight line would pass over us to N; but as it falls on the atmosphere at M, and in a very oblique direction, the angle F M S being very acute, it will thence undergo a very considerable refraction; and instead of proceeding forward to N, would assume the direction M O, so that the sun would be actually visible to a person at O, though still considerably below the horizon at S; or, which is the same thing, below the horizontal line O M V.

Fig. 211.



5. However, as the ray M O, which meets the eye, is horizontal, we assign that direction to the sun himself, and imagine him to be actually at V, that is, in the horizon, though he is still below it.

And, reciprocally, as often as we see the sun, or any star, in the horizon, we are assured they are still below it, according to the angle S M V, which astronomers have observed to be about half a degree, or, more exactly, 32 minutes.

6. In the morning, then, we see the sun before he has reached our horizon, that is, while he is yet an angle of 32 minutes below it; and in the evening a considerable time after he is really set, as we see him till he has descended an angle of 32 minutes. We call that the true rising and setting of the sun when he is actually in the horizon; and the commencement of his appearance in the morning and disappearing at night we denominate the apparent rising and setting.

7. This refraction of the atmosphere, which renders the apparent rising and setting of the sun both earlier and later than the real, procures for us the benefit of a much longer day than we should enjoy did not the atmosphere produce this effect. Such is the explanation of a very important phenomenon in nature.

18*th May*, 1762.

LETTER CXIX.

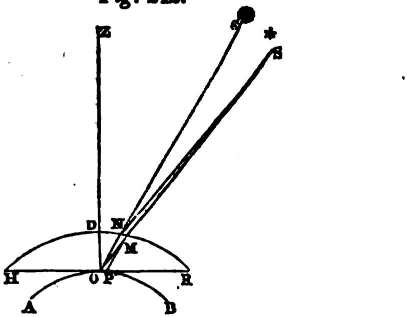
The Stars appear at a greater Elevation than they are. Table of Refractions.

You have now, no doubt, a clear idea of this singular effect of our atmosphere, by which the sun and the other heavenly bodies are rendered visible in the horizon, though considerably below it, whereas they would be invisible but for the refraction. For the same reason the sun, and all the heavenly bodies always appear at a greater elevation above the horizon than they really are. It is necessary, therefore, carefully to distinguish the

apparent elevation of a star from what it would be were there no atmosphere. I shall endeavour to set this in the clearest light possible.

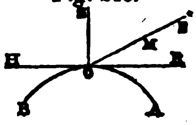
1. Let the arch A O B, *Fig.* 212, be part of the surface of the earth, and O the spot where we are, through which draw the straight line H O R, touching the surface, and this line H O R will represent the true horizon. From O let there be drawn perpendicularly the straight line O Z, which is the same thing as suspending a given weight by a cord. This line is said to be vertical, and the point Z of the heavens, in which it terminates, is called the zenith. This line, O Z, then, is perpendicular to the horizontal line H O R, so that one being known, the other must be known likewise.

Fig. 212.



2. This being laid down, let there be a star at S, *Fig.* 213: were there no atmosphere, the ray S M O would proceed in a straight line to the eye at O, and we should see it in the same direction O M S, where it would actually be—that is, we should see it in its true place. Let us then mea-

Fig. 213.



sure the angle S O R, formed by the ray S O with the horizon O R, and this angle is named the height of the star, or its elevation above the horizon. We measure also the angle S O Z, formed by the ray S O with the vertical line O Z terminating in the zenith: and as the angle Z O R is a right angle, or 90 degrees, we have only to subtract the angle S O Z from 90 degrees to have the angle S O R, which gives the true elevation of the star.

3. But let us now attend to the atmosphere, which I suppose terminated by the arch H D N M R, *Fig.* 212, p. 418, and I remark, first, that the preceding ray S M of the star S, on entering into M in the atmosphere, does not proceed directly forward to the eye at O, but, from the refraction, will assume another direction, as M P, and consequently will not meet the eye at O: so that if this star sent down to the earth only that ray S M, to a person at O it would be absolutely invisible. But it must be considered that every luminous point emits its rays in all directions, and that all space is filled with them.

4. There will be, then, among others, some ray, as S N, which is broken or refracted on entering the atmosphere at N; so that its continuation N O shall pass precisely to an eye at O. The refracted ray N O is not, therefore, in a straight line with the ray S M; and if N O be produced forward to *s*, the continuation N *s* will form an angle with the ray N S, namely, the angle S N *s*, which is what we call the refraction, and which is greater in proportion as the angle S N R, under which the ray S N enters into the atmosphere, is more acute, as was demonstrated in the preceding Letter.

5. It is the ray N O, consequently, which paints in the eye the image of the star S, and which renders it visible: and as this ray comes in the direction N O, as if the star were in it, we imagine the star likewise to be situated in the direction N O, or in

that line continued somewhere at *s*. This point *s* being different from the real place of the star S, we call *s* the apparent place of the star, which must be carefully distinguished from its place S, where the star would be seen were there no atmosphere.

6. Since, then, the star is seen by the ray N O, the angle N O R, which this ray N O makes with the horizon, is the apparent altitude of the star; and when by a proper instrument we measure the angle N O R, we are said to have found the apparent altitude of the star; the real altitude being, as we have shown, the angle R O S.

7. Hence it is evident, that the apparent altitude R O N is greater than the real altitude R O M, so that the stars appear to us at a greater elevation above the horizon than they really are; for the same reason they appear already in the horizon while they are still below it. Now, the excess of the apparent altitude above the true is the angle M O N, which does not differ from the angle S N *s*, and which we call the refraction. For, though the angle S N *s*, as being the external angle to the triangle S N O, is equal to the two internal and opposite angles taken together, namely, S O N and N S O, we may consider, on account of the immense distance of the stars, the lines O S and N S as parallel, and consequently the angle O S N vanishes; so that the angle S O N is nearly equal to the angle of refraction S N *s*.

8. Having found, then, the apparent altitude of a star, you must subtract from it the refraction, in order to have the real altitude, which there is no other method of discovering. For this purpose, astronomers have been at much pains to ascertain the refraction to be subtracted from each apparent altitude, that is, to determine how much must be deducted in order to reduce the apparent to the real altitude.

9. From a long series of observations, they have been at length enabled to construct a table, called the table of refraction, in which is marked for every apparent altitude the refraction or angle to be subtracted. Thus, when the apparent altitude is nothing, that is, when the star appears in the horizon, the refraction is 32 minutes; the star is accordingly an angle of actually 32 minutes below the horizon. But if the star has acquired any degree of elevation, be it ever so inconsiderable, the refraction becomes much less. At the altitude of 15 degrees it is no more than four minutes; at the altitude of 40 degrees it is only one minute; and as the altitude increases, the refraction always becomes less, till at length it entirely disappears at the altitude of 90 degrees.

10. This is the case when a star is seen in the very zenith; for its elevation is then 90 degrees, and the real and apparent altitude is the same: and we are fully assured that a star seen in the zenith is actually there, and that the refraction of the atmosphere does not change its place, as at every other degree of altitude.

THE END.

GLOSSARY

OF

FOREIGN AND SCIENTIFIC TERMS.

FROM THE LONDON EDITION, REVISED.

A.

ABERRATION, in astronomy, an apparent motion in the celestial bodies, occasioned by the progressive motion of light, and the earth's annual motion. Latin.

Abstraction, in metaphysics, that operation of the mind which pursues a general idea without attending to the particulars of which it is made up. Thus, man, tree, are *abstract* ideas, and may be pursued without descending to any one individual person or plant included in the general term. Accordingly, all qualities, such as whiteness, cruelty, generosity, are *abstract* ideas. Latin.

Accord, in music, the same with *concord*, the relation of two sounds which are always agreeable to the ear, whether emitted at once or in succession. Latin.

Achromatic Glasses, in optics, are those which bring diffused rays of light to a focus, and form an image free from any unnatural colour. The word is of Greek extraction, and signifies *colourless*.

Aeriform, having the form or consistency of air. Latin.

Aerostation, the art of ascending into the atmosphere by means of a balloon filled with air or gas lighter than that of the atmosphere. Latin.

Affirmative proposition, in logic, a proposition which asserts or affirms; as, Man is mortal. Latin.

Air-pump, a machine for making experiments on air, chiefly by exhausting close vessels of that fluid.

Algebra, the science of universal arithmetic; the general process of which is, by comparing supposed and unknown numbers or quantities with such as are known, to reduce supposition to certainty. Arabic.

Alkali, in chymistry, a substance which turns vegetable blues to green, and unites with oils and forms soap, and with acids and forms salts. Arabic.

Altitude, in astronomy, the height of a heavenly body above the horizon. Latin.

Amalgamate, to incorporate mercury or quicksilver with other metals, sometimes used to denote, in general, the mixture and consolidation of several substances, so as to make them appear one. Greek.

Analogous, having resemblance or agreement. Greek.

Analysis, resolution into first principles, whether in grammar, logic, mathematics, or chymistry. In grammar, an *analysis* of a sentence is an indication of the various parts of speech of which it is composed, and the grammatical rules according to which they are arranged. A *chymical analysis* is the decomposition of a body for the purpose of ascertaining its elementary or constituent parts. Greek.

Anathema, and its compounds, something set apart to a sacred use;—generally used in an ungracious sense; devoted to destruction, accursed. Greek.

Anatomy, the science which treats of the structure of the body, and the art of dissecting and reasoning upon it. Greek.

Angle, the opening of two lines which meet in a point, so as not to form of both one straight line. Latin.

Antecedent, in logic, the former of two propositions in a species of reasoning, which, without the intervention of any middle proposition, leads directly to a fair conclusion; and this conclusion is termed the *Consequent*. Thus—I reflect; therefore I exist. "I reflect" is the *antecedent*, "therefore I exist" is the *consequent*. Latin.

Antipodes, the inhabitants of the globe diametrically opposite to us, and whose feet point exactly to our feet. Greek.

Aperture, opening. Latin.

Approximation, a coming nearer to. In astronomy, the gradual approach of two celestial bodies towards each other. In arithmetic, a nearer approach to a number or root sought, without the possibility of arriving at it exactly. Latin.

Aqueduct, that which conveys or conducts water. A pipe, a canal. Latin.

Aqueous, watery, consisting of water. Latin.

Arithmetic, the science of numbers. Greek.

Astronomy, the science of the heavenly bodies. Greek.

Astrology, the pretended science of predicting future events by means of the planets. Greek.

Atmosphere, the body of air which surrounds the globe on all sides. Greek.

Axis, in geography, an imaginary straight line passing through the centre of the earth from pole to pole, round which the globe revolves once every twenty-four hours. Latin.

B.

Barometer, an instrument of glass filled with mercury, which indicates the pressure of the air, and which is in general use as an index of the weather. The word is Greek, and signifies *weight-measurer*.

Bisect, to cut into two equal parts. Latin.

Bituminous, like to or consisting of *bitumen*,—a fat, clammy, easily-inflammable juice, impregnating coal, or scummed off lakes. Latin.

Bomb, a hollow cast-iron globe, to be thrown from a species of great gun called *mortar*, and intended to burst by the force of gunpowder at the moment of falling, and to scatter destruction all around. The term is in this work employed to explain the path of all bodies forcibly thrown through the air, and the effect of gravity in bringing all heavy moving bodies to the ground. Latin.

Botany, the science of plants, or that part of natural history which has the vegetable world for its object. Greek.

C.

Camera Obscura, an apartment darkened, all but a small circular opening, to which a double-convex glass is fitted, and by which external objects are represented in their natural colours, motions, and proportions, on a white skreen within the apartment. Latin.

Cataract, a body of water precipitated from a great height. Greek.

Catoptrics, that branch of the science of vision which relates to reflected light. The reflective properties of all bodies through which we cannot see, but which throw back the light, belong to catoptrics, such as mirrors of every kind. The word is Greek, and signifies *backward vision*.

Cavity, a hollow. Latin.

Causa sufficiens, sufficient or satisfying cause or reason, a jargon employed by certain metaphysicians of the last age, who attempted to check all rational experimental inquiry by calling continually for the *causa sufficiens*, or adequate cause, of every fact that occurred; while they were bewildering themselves, and attempting to bewilder mankind, in a philosophical maze useless, reasonless, and therefore unsatisfactory.

Centre, a point within a circle or sphere equally distant from every part of the circumference or surface. Latin.

Chart, a delineation on paper of part of the land or of the sea, or both. Latin.

Chimera, a vain and wild imagination. Latin.

Choral Music, a sacred band composed of voices and instruments. Latin.

Chromatic, in optics, relating to colour: in music, to a certain series of sounds. Greek.

Chymistry, the science which treats of the composition of matter in its various conditions, of the nature of its elementary principles, and of the intimate affinities of simple and compound bodies.

Circle, a round figure having the essential property that every point of its surrounding line, called the circumference, shall be equally distant from its middle point, called the centre. Latin.

Circumambient, encompassing, surrounding; applied particularly to air and water. Latin.

Cohesion, that species of attraction which unites the particles of bodies, and produces solidity in its various degrees. Latin.

Collision, the clashing of one body against another. Latin.

Comet, a body with a luminous train, like flowing hair, averted from the sun; of uncertain appearance and reappearance, but undoubtedly forming part of our solar system. Greek.

Complex, made up of various qualities or ingredients. A beautiful, wise, and good woman, is a *complex* idea, containing three distinct ideas—beauty, wisdom, goodness: it might be rendered still more complex by the addition of highbòrn, rich, religious.

Compression, the act of reducing to a smaller space by pressure.

Concave, the hollowed surface of a curvilinear body. Latin.

Concussion, mutual shock, by the violent meeting of two bodies. Latin.

Condensation, the act of forcing matter into a smaller space. Latin.

Congelation, the reduction of a fluid to a solid substance, as water to ice, by cold. Latin.

Concentric Circles, one within another, having a common centre. Latin.

Conical, having the form of a *cone*, which is a figure produced by turning round a right-angled triangle about its perpendicular side; a common candle-extinguisher conveys the idea of it. Greek.

Consequent. See *Antecedent*. The two terms are what is called *correlative*; in other words, the one cannot be understood but by referring to the other.

Consonance, in music, the agreement of two sounds emitted at the same time. Latin.

Constituent, contributing to make up or compose. Thus the constituent parts of gunpowder are saltpetre, sulphur, and charcoal. Latin.

Continuity, uninterrupted connexion; the unviolated union of the parts of an animal body. Latin.

Contexture, an interweaving. Latin.

Contour, the extreme bounding line of any object. Children delineate the contours of each other's faces by tracing with a pencil the line described on the wall when the face is placed between a light and the wall. French.

Convergent, gradually approaching. Placed at the extremity of an avenue of two rows of trees, planted in straight lines, equally distant throughout, you perceive them apparently approaching, and at length almost meeting; they are apparently convergent.

Convex, the prominent or swelling surface of a curvilinear body. Latin.

Cornea, the transparent portion of the external coat of the eye. Latin.

Corporeal, belonging to body. Latin.

Corpus Callosum, in metaphysics and anatomy, the part of the human brain where the soul is supposed to reside. Latin, but of ludicrous derivation.

Corpuscle, a small or minute body. Latin.

Couching, an operation in surgery, that consists in removing the opaque lens out of the axis of vision, by means of a needle constructed for the purpose.

Crucible, a pot which can stand fire, employed in melting and refining metals. Low Latin.

Crystalline, the solid, transparent, internal humour of the eye. It is a double-convex lens, situated immediately behind the pupil. Its occasional opacity produces the disease called *cataract*. Greek.

Cube, and its compounds, a figure square and rectangular in all its dimensions and situations. A common die conveys the idea of it. A cubical room of twenty feet is a room twenty feet long, twenty feet broad, and twenty feet high, and all in straight lines, and at right angles. Greek.

Curve, a bending line. Latin.

Cylinder, a figure formed by turning a parallelogram round one of its sides as an axis. The barrel of a hand-organ is a cylinder. The word is derived from a Greek verb which signifies to whirl round.

D.

Decompose, to separate things compounded. Thus, in printing, to compose is to arrange the types in a frame, in the order of words and sentences; and to decompose is to take the frame to pieces. Latin.

Degree, in geography, the three hundred and sixtieth part of the circumference of the globe. It contains about sixty-nine English miles. French.

Density, comparative solidity. Latin.

GLOSSARY.

Dephlogistic, deprived of fiery, inflammable qualities. Greek.
Detonation, the thunder-like noise produced by explosions. Latin.
Diagram, a figure delineated for the purpose of demonstration and planation. Greek.
Diameter, a straight line drawn through the centre of a circle or gla Greek.
Diaphonous body, that which easily transmits the light, as gla Greek.
Diaphragm, in optical instruments, a circular piece of pasteboard, o other non-transparent substance, applied to the object-glass to ex clude part of the rays of light. Greek.
Diatonic, an epithet given to the common music, as it proceeds by tones both ascending and descending. Greek.
Dilate, to expand, to spread over greater space. Latin.
Dimension, measure. Latin.
Dioptrics, that branch of the science of vision which relates to the transmission of the rays of light through transparent bodies. Greek.
Dissonance, in music, sounds which do not harmonise, but are harsh and disagreeable to the ear. Latin.
Distraction, tendency in different directions. Latin.
Divergent, straight lines gradually removing farther and farther from each other. See *Convergent*. Latin.
Diving-bell, a machine of wood, glass, or metal, in form of a bell, for the purpose of enabling persons employed in certain kinds of fishery, and in recovering goods lost by shipwreck, to descend and remain with safety under water.
Divisibility, capability of being divided. Latin.
Double-concave, an optical glass which has both surfaces hollowed.
Double-convex, an optical glass which has both surfaces raised.
Ducat, a ducal coin of gold, current in Southern Europe, value about two dollars and ten cents.
Ductile, pliant, easily drawn or spread out. Latin.

E.

Effulgence, lustre, brightness. Latin.
Elasticity, a power in bodies of recovering their former situation as soon as the force is removed which had changed it. Thus, the extremities of a bow are brought nearer by drawing the string; but when the string is relaxed, the bow, by its elasticity, is restored to its natural state. It is a property of air, as well as of solid bodies. Greek.
ectricity, the disposition which certain bodies have of acquiring, by rubbing, the quality of attracting other bodies, and of emitting sparks of fire. It is derived from a Greek word signifying *amber*, which is one of the substances endowed with the electrical virtue.
it, to strike out by force. Thus, by a sharp stroke of the steel on lint, fire is elicited. Latin.
um, or *Eulogium*, an oration in praise of one absent or dead. reek.
lation, the act of explaining or rendering clearer. Latin.
ation, an issuing or flowing from any substance as a source. tin.
on, in astronomy, the reappearance of a star, planet, or satellite, having been obscured by the intervention of another body inter- ting the light. Latin.

Emission, the act of sending out, or giving vent. Latin.

Encyclopedia, the whole circle of science; a universal scientific dictionary. Greek.

Epicurean, belonging to the doctrine or philosophy of Epicurus; according to which man's duty and happiness are made to consist in reasonable indulgence; it has become descriptive of refined luxury.

Equator, an imaginary great circle, equally distant from both poles, surrounding the globe from east to west, and dividing it into the Northern and Southern hemispheres. On maps the degrees of longitude are marked on it, from 1 to 180 east and west of the first meridian. It is by way of distinction called the LINE. Latin.

Equidistant, at equal distances. Latin.

Equilibrium, exactness of balance or counterpoise. Latin. The ablative with the preposition is adopted into our language, *in equilibrio*, to express perfectness of equality in opposed weights.

Equinox, the equalization of day and night which takes place twice every year, the 21st of March and the 21st of September, when the sun, in his alternate progress from north to south, and from south to north, passes directly over the equator, which is likewise, for this reason, frequently denominated the Equinoctial Line. Latin.

Era, an important event or period of time. Latin.

Erudition, extensive and profound learning. Latin.

Ether, the most subtile and attenuated of all fluids. Greek.

Evaporation, the act of flying off in fumes or vapour.

Exhalation, a word of the same import with the preceding; evaporation may be considered as the cause, and exhalation as the effect. Latin.

Expansibility, capability of being spread out, and of occupying a larger space. Latin.

Experiment, a practical trial made to ascertain any fact. Latin.

Extension, space over which matter is diffused; size, magnitude. Latin.

Extraneous, not belonging to. Latin.

F.

Fathom, a measure of length containing six feet. Saxon.

Fibre, a small thread. In anatomy, fibres are long, slender, whitish filaments, variously interwoven, which form the solid parts of an animal body. Latin.

Fifth, in music, one of the harmonic intervals or concords, and the third in respect of harmony, or agreeableness to the ear. It is so called because it contains five tones or sounds between its extremes. See vol. i. let. vii.

Filament, the same with *fibre*. Latin.

Fluid, consisting of parts easily moveable among each other, as melted metals, water, air. Latin.

Flux, in geography, the rising of the tide. Latin.

Focus, in optics, the little circle in which rays of light are collected, either after passing through a glass, or on being thrown back from a reflector, and where they exert their greatest power of burning. Latin.

Formula, a set or prescribed standard; a scheme for solving mathematical and algebraical questions. Latin.

Forte, in music, forcibly, in opposition to *piano*, softly. Latin.

Fourth, in music, one of the harmonic intervals, and th
spect of agreeableness to the ear. It consists of two
in the proportion of 4 to 3; that is, of sounds prod
whose lengths are in the proportion of 4 to 3. Se
and vii.

Friction, the act of rubbing one body against another. L

Fusible, that may be melted. Latin.

G.

Gamut, the scale of musical notes. Italian.

Genus, kind, general class containing several species, whic
tain many individuals. Thus, dog is a *genus*, gre
species, and Lightfoot an *individual*. Latin. The plu

Geography, a description of the earth. Greek.

Geometry, the science of quantity, magnitude or extension
considered. Greek.

Glaucous, azure-coloured. Greek.

Globule, small globe; little particles of a spherical form. La

Gradation, regular progress from one step to another. Latin.

Gravity, weight; in the system of the universe, that princi
bodies which attracts them towards each other. Latin.

Groove, a channel cut out in a hard body with a tool, fitted
body which is designed to move in it.

H.

Harmony, in music, a combination of sounds perfectly adapte
other, so as to produce a pleasing effect on the ear. Greel

Hemisphere, one-half of a globe. Greek.

Heterogeneous, composed of dissimilar or discordant parts;
opposite of *homogeneous*, which signifies, composed of t
ilar. Greek.

Horizon, the line which terminates the view. In geography,
nany circle encompassing the globe, and dividing it into
and under hemispheres. To a person placed at either of
the equator would be the real horizon. The sensible hori
circle visibly surrounding us, where the sky and the earth
meet. Greek.

Humidity, moisture. Latin.

Hydrography, a description of that part of our globe which c
water. Greek.

Hypothesis, a proposition or doctrine supposed to be true, bu
confirmed by irresistible argument or satisfying experiment

I.

alist, a kind of philosopher, who denies the existence of m
reduces every thing to idea or mental image. Greek.

mitable, what admits of no bound. Latin.

mination, the act of diffusing light. Latin.

sion, what deceives by a false appearance. Latin.

aterial, in philosophy, not consisting of body or matter.

ersion, in astronomy, the disappearance of a celestial bo
nterception of its light by another body. Latin.

Impenetrability, that property of all bodies in virtue of which no two can occupy the same space at the same time. Latin.

Impulsion, the agency of one body in motion upon another. Latin.

Imputability, the quality of being charged upon, or ascribed unto. Latin.

Incidence, the direction in which one body falls upon or strikes another; and the angle formed by that line and the plane struck upon is called the angle of incidence. Latin.

Index, the fore-finger; any instrument that points out or indicates. Latin.

Individual, one separate, distinct, undivided whole.

Inertia, that quality of bodies in virtue of which they are disposed to continue in a state of rest when at rest, or of motion when in motion; and which can be overcome only by a power not in the body itself. Latin.

Infinity, boundlessness, applied equally to space, number, and duration; *in infinitum*, without limit, without end. Latin.

Inflection, the act of bending or turning. Latin.

Inherent, naturally belonging to, and inseparable from. Latin.

Intellectual, relating to the understanding, mental. Latin.

Intensity, the state of being stretched, heightened, affected to a very high degree. Latin.

Interception, the cutting off or obstruction of communication. Latin.

Intersect, mutually to cut or divide. Latin.

Interstice, the space between one thing and another.

Inverse, having changed places, indirect, turned upside down. Latin.

Iris, the circle round the pupil of the eye. Latin.

L.

Labyrinth, maze, inextricable difficulty or perplexity. Latin.

Latitude, in geography, distance of places from the equator measured on the meridian, in degrees and minutes. The degree contains about 69 English miles, and a minute is the sixtieth part of a degree. The highest possible degree of latitude is at the poles, each being 90 degrees from the equator. Latin.

Lens, a glass for assisting vision, or deriving fire from the collected rays of the sun.

Lenticular, having the form of a lens.

Level, being at the same height in all parts. Saxon.

Literati, the learned; the plural of the Latin word *literatus*, a learned man.

Logic, the art of right reasoning, for the purpose of investigating and communicating useful truth. Greek.

Longitude, in geography, the angle which is formed by the meridian of any place and the first meridian, measured in degrees and minutes on the equator. Latin.

Lunar tide, the flowing and ebbing of the tide relatively to the moon. Latin.

Lymphatic vessels, slender transparent tubes through which *lymph*, or a clear colourless fluid, is conveyed.

M.

Magnet, or *loadstone*, an ore of iron which attracts iron and steel, and gives polarity to a needle. Art has been enabled, by means of bars

of steel, successfully to imitate the natural
Latin.

Magnitude, greatness, bulk, extension. Latin.

Manichean, one of a sect who maintained the existenc
spirit.

Major, in logic, the first proposition of a syllogism
general assertion or denial; as, all men are mort
fect. Latin.

Materialist, one who denies the existence of spi
Latin.

Mathematics, the science which has for its object eve
being measured or numbered. Greek.

Mean, or ***Medium***, in physics, that which intervenes
stance and another; in logic, an intermediate pro
to lead to a fair and just conclusion. Latin.

Mechanics, the geometry of motion; the science of co
machinery. Greek.

Membrane, a web of various fibres interwoven, for w
parts of vegetable and animal bodies. Latin.

Meniscus lens, in optics, a glass which is convex o
concave on the other, the two surfaces approachi

Mephites, poisonous, ill-scented vapour. Latin.

Mercury, the chymical name of the fluid commonly

Meridian, in geography, a great circle encompassing
direction of South and North, and dividing it
western hemispheres. The degrees of latitude,
both poles, are marked on this circle. Every
comes to its meridian once in every twenty-four
its instant of noon. Latin.

Metaphysics, otherwise called *Ontology*, the science o
beings in general. It employs abstract reasoni
Greek.

Meteorology, the science of meteors, that is, of bodies
and quickly passing away. Greek.

Microscope, an optical instrument, which, by means
nifying glass, renders distinctly visible objects
unassisted eye. Greek.

Minor, in logic, the second, or particular proposition
example, in this syllogism,—

All men are mortal:
But, The king is a man;
Therefore, The king is mortal.

the first proposition, "All men are mortal," is the
"The king is a man," is the *minor;* and these
premises; the third, "the king is mortal," is the c

Mobility, easiness of being moved. Latin.

Mode, in logic, particular form or structure of argume

Monad, a minute particle of matter which admits of
vision. Greek.

Monochord, a musical instrument of one string. Gre

Myops, short-sighted. Greek.

N.

Nadir, the point in the heavens directly under foot.

Navigation, the art of sailing. Latin.

Negation, denial, the opposite of *affirmation*. Latin.

Notion, thought; representation of any thing formed by the mind. Latin.

O.

Objective lens, in optics, that glass of a telescope which is turned to the object or thing looked at. Latin.

Oblique, not direct, not perpendicular, not parallel. Latin.

Observatory, an edifice reared for the purpose of astronomical observations. Latin.

Occult, secret, unknown, undiscoverable. Latin.

Octave, in music, a regular succession of notes from one to eight; the first and the eighth having the same name and emitting the same sound. Latin.

Ocular lens, in optics, that glass of a telescope which is applied to the eye. Latin.

Opaque, impervious to the rays of light, not transparent. Latin.

Optics, the science of the nature and laws of vision, or sight. Greek.

Orb, sphere, heavenly globular body. Latin.

Orbit, the circular path in which a planet moves round the sun or another planet. Latin.

Oscillation, alternate moving backward and forward, like the pendulum of a clock. Latin.

P.

Paradox, a tenet which exceeds or contradicts received opinion; affirmation contrary to appearance. Greek.

Parallel lines, in geometry, lines which through the whole of their length maintain the same distance. They are the opposite of *convergent* and *divergent*. Latin.

Parallelism, state of being parallel.

Parallelogram, a geometrical figure of four sides, having this property, that the opposite sides are equal and parallel, and the opposite angles equal. Greek.

Pellucid, transmitting the rays of light, transparent. Latin.

Pendulum, a body suspended so as to swing backwards and forwards without obstruction. A pendulum is generally used for measuring time; the great perfection of such an instrument is, that every vibration or swing shall be performed in exactly the same quantity of time. Latin.

Perception, the power of perceiving, knowledge, consciousness. Latin.

Permeable, susceptible of being passed through. Latin.

Perpendicular, in geometry, one line standing on another, or on a horizontal plane, without the slightest inclination to one side or the other, and forming right-angles with the horizontal line or plane. Latin.

Phalanx, a military force closely imbodied. Latin.

Phasis, appearance presented by the changes of a heavenly body, particularly those of the moon. The plural *phases* is adopted in our language. Greek and Latin.

Phenomenon, striking appearance in nature. The plural *phenomena* is in common use. Greek.

Philosophy, knowledge natural or moral; system in correspondence to

which important truths are explained; academical co
Greek.

Physics, the science of nature, natural philosophy. Gre
Piano, in music, softly, delicately, opposite to forte. Ital
Piston, the moveable circular substance fitted to the cavi
such as a pump or syringe,—for the purpose of suct
or condensing of fluids. French.
Planet, a wandering star; those heavenly bodies, our gl
which perform their courses round the sun are called p
Plano-concave, in optics, a glass which has one surface
other hollow. Latin.
Plano-convex, an optical glass which has one surface
other raised. Latin.
Plenum, space filled with substance. Latin.
Plumb-line, a weight appended to a string, for the purpos
ing perpendicularity.
Polar Circles, circles parallel to the equator and the trop
tance of twenty-three degrees and a half each from
pole. Latin.
Polarity, tendency towards the pole. Latin.
Polygon, a figure having many sides and angles. Greek.
Polytheism, the doctrine of a plurality of gods. Greek.
Porous, full of small minute spaces. Greek.
Presbytes, far-sighted. Greek.
Prescience, foreknowledge. Latin.
Predicate, in logic, what is affirmed of the subject, as, m
Latin.
Predilection, preference given from preconceived affection
Pre-established Harmony, the metaphysical doctrine o
adaptation of mind to matter, by a creative act of
Will, in virtue of which every human action is perfo
Prism, a triangular optical instrument of glass, contrived f
of making experiments with the rays of light. Gree
Problem, a proposition announcing something to be first p
then demonstrated. Greek.
Proboscis, the snout or trunk of an elephant or other anim
Prominent, jutting out, projecting forward. Latin.
Proposition, a point advanced or affirmed with a view to p
Proximity, nearness. Latin.
Pupil, in optics, the apple or central opening of the eye.
Pyrometer, a machine contrived to ascertain the degree of
of solid bodies by the force of fire. Greek.
Pyrrhonist, a universal doubter and unbeliever; derived f

Q.

Quadrant, the fourth part of a circle; an instrument of th
trived to measure altitudes and distances of celestial bo
Quadrilateral, consisting of four sides. Latin.
Quotient, in arithmetic, the number resulting from the di
numbers which measure each other. Thus, on dividi
have a quotient of 9.

R.

Radius, in English ray, a straight line drawn from the centre of a circle or sphere to the circumference. The plural *radii* is in use. Latin.

Rarefaction, the rendering of a substance thinner, more transparent; it is the opposite of condensation. Latin.

Ratio, proportion. Latin.

Ratiocination, a process of reasoning, a deduction of fair conclusions from admitted premises. Latin.

Recipient, that which receives and contains. Latin.

Reciprocally, mutually, interchangeably. Latin.

Rectangular, containing one or more right-angles. A right-angle consists of 90 degrees.

Rectilinear, consisting of straight lines. Latin.

Reflection, in catoptrics, the sending back of the rays of light from an opaque surface. Latin.

Reflux, the ebbing, or flowing back of the tide. Latin.

Refraction, in dioptrics, the deviation of a ray of light on passing obliquely from one medium into another of a different density, as from air into water or glass. Latin.

Refrangibility, disposition to leave the direct course, capability of being broken or refracted. Latin.

Refrangent medium, that which alters or breaks off the course of rays. Latin.

Reminiscence, the power of recollection, memory. Latin.

Repulsion, the act or power of driving back. Latin.

Resinous, consisting of, or similar to, resin, a principle contained in certain vegetables. Latin.

Resonance, sound repeated. Latin.

Respiration, the act of breathing. Latin.

Reticulated, formed like a net. Latin.

Retina, the delicate net-like membrane at the bottom of the eye, on which are painted the images of the objects which we contemplate. Latin.

Retrograde, moving in a backward direction. Latin.

Reverberation, the act of beating or driving back. Latin.

Revery, loose, wild, irregular meditation.

S.

Satellite, an inferior attendant planet revolving round a greater. Latin.

Scalpel, a surgical dissecting-knife. Latin.

Science, knowledge: grammar, rhetoric, logic, arithmetic, music, geometry, astronomy, have been styled the seven liberal arts.

Segment, in geometry, part of a circle formed by a straight line drawn from one extremity of any arc to the other, and the part of the circumference which constitutes that arc. The straight line is denominated the chord of the arc, from its resemblance to a bowstring.

Semicircle, the half of a circle; the segment formed by the diameter as the chord, and one-half the circumference as the arc. Latin.

Semitone, in music, half a tone, the least of all intervals admitted into modern music. The semitone major is the difference between the greater third and the fourth; its relation is as 15 to 16. The semitone minor is the difference between the greater third and the lesser third, and its relation is as 24 to 25. Latin.

Sensation, perception by means of the senses. Latin.

Series, regular, settled, proportional order of progressi
bers, 9, 18, 27, 36, 45, 54, 63, are in a series. The v
singular and plural. Latin.

Seventh, in music, the inverted discordant interval of t
by the ancients *Heptachordon*, because it is formed
There are four sorts of the seventh, of which the f
proportions: as 5 to 9, as 8 to 15, as 75 to 128, as 8
harsh and unharmonious.

Solar tide, the flux and reflux of the tide relatively to th

Solution. demonstration, clearing up of intricacy or dif

Sonorous, emitting loud or shrill sounds. Latin.

Species, kind, sort, class. See *Genus*. It is the same
plural. Latin.

Spectrum, an image, a visible form. Latin.

Sphere, globe. Greek.

Spheroid, approaching to the form of a sphere. If
called a *prolate*, if flattened, an *oblate* spheroid. C

Spiritual, not consisting of, but distinct from, matter o

Sublime, elevated in place. In chymistry, raised by
Latin.

Subterfuge, a paltry escape or evasion. Latin.

Subterraneous, under the surface of the ground. Lati

Subtile, thin, not dense, not gross. Latin.

Superficial, external, extended along the surface. Lati

Supernatural, what is above or beyond the ordinary c
Latin.

Surface, in geometry, length and breadth without thick

Syllogism, in logic, an argument consisting of three p
example, Every virtue is commendable; honesty is
fore honesty is commendable. See *Major* and *Min*

System, a scheme of combination and arrangement, whi
things to a regular connexion, dependence, and co-o

T.

Tangent, in geometry, a straight line touching a circl
single point. Latin.

Telescope, an optical instrument designed, by the mag
glasses, to represent distant bodies as much nearer

Temperament, state of body or mind as produced by, or
the predominancy of a particular quality. Latin.

Tension, the state of being stretched out, wound up, d

Tenuity, thinness, delicate fineness. Latin.

Term, descriptive name or phrase, component part, con

Terraqueous, consisting of land and water. Latin.

Theology, systematic divinity. Greek.

Theorem, a proposition announced for demonstration.

Theory, a doctrine contemplated and conceived in the n
confirmed by irresistible argument or satisfying exp

Thermometer, an instrument contrived to measure the
other body by means of the rising or falling of a
Greek.

Third, in music, the first of the two imperfect concords,
its interval is always composed of two degrees
sounds. The tierce major, or greater third, is repr

bers by the ratio of 4 to 5; and the lesser by the relation of 5 to 6 See vol. i. let. vi. and vii.

Tide, the alternate rising and falling of the water in rivers and along the shores of the sea. Saxon and Dutch.

Tone, in music, the degree of elevation which the voice assumes, and to which instruments are adapted, in order to the harmonious execution of a musical composition; a pitch-pipe. Latin and Greek.

Transit, in astronomy, the passing of one heavenly body over the disk of another. Latin.

Transmission, the sending of one body or substance through or to another. Latin.

Transparent, clear, permeable to light, as air, water, glass. Latin.

Transverse, in a cross direction. Latin.

Triangle, a geometrical figure consisting of three sides and three angles. Latin.

Tube, a pipe, a long hollow body. Latin.

Tunicle, a small coat or covering. Latin.

U.

Ultimate, final, beyond which there is no farther progress. Latin.

Unison, emission of the same or harmonious sounds. Latin.

Untenable, what cannot be maintained or supported.

V.

Vacuum, empty space. Latin.

Valve, a door, a moveable membrane in the vessels of an animal body, and imitated by art in the construction of various machines, which opens for giving passage to fluids in one direction, but shuts to oppose their return through the same passage. Latin.

Velocity, speed, swiftness of motion. Latin.

Vertical, perpendicular, upright. Vertical angles, in geometry, are those formed by the intersection of two straight lines, in whatever direction, making four in all at the point of intersection, and of which the opposite two and two are equal. Latin.

Vibration, motion backwards and forwards. Latin.

Visual, relating to vision or sight, belonging to the eye. Latin.

Vitreous, composed of or resembling glass. Latin.

Vivid, lively, brisk, sprightly. Latin.

W.

Waning, gradual diminution of apparent magnitude and light. Saxon.

Waxing, gradual increase of apparent magnitude and light, particularly of the moon. Saxon and Danish.

Wind-gun, a gun which forcibly emits a ball by means of compressed air or wind.

Z.

Zenith, the point in the heavens exactly over-head; the opposite of *Nadir*.

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